The Huxley Lecture

ON

RECENT ADVANCES IN SCIENCE IN RELATION TO PRACTICAL MEDICINE.

SOME PROBLEMS IN INFECTION AND ITS CONTROL.

DELIVERED IN THE CHARING CROSS HOSPITAL

BY

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I EXPERIENCE a high sense of honour on this occasion, with which is mingled no less trepidation, in view of the master in whose memory this lectureship was founded, and the great names that in the past have been linked with the post I am to-day asked to fill. I must believe that Huxley would have felt a deep interest in the theme which I have chosen to discuss before you, and would have found in its intrinsic importance a compensation for any shortcoming that may appear in the presentation. For Huxley evinced a penetrating appreciation of that branch of biological science that has come to be called bacteriology, and as President of the British Association in 1870 devoted his address to an illuminating examination of the doctrine of abiogenesis, or spontaneous generation, *versus* the doctrine of biogenesis, or descent from living ancestors. This subject, long holding a merely academic interest, had become, in the two decades immediately preceding, the ground over which the conflict raged, and out of which was to emerge the modern science of microbiology. While Huxley clearly pointed out that Redi in the seventeenth century and Spallanzani in the eighteenth had delivered the first telling blows that later, through Pasteur, led to the overwhelming defeat of the spontaneous generationists and the establishment on an indisputable basis of the extrinsic origin of the contagious and infectious diseases, he did not fail to perceive in the discoveries just being made in reference to fermentation, putrefaction, and certain fungus and other diseases of insects, the herald of the new science that was to throw its protecting mantle, not about men alone, but about all the higher animals and even about the plants, in order that the useful and indispensable should be protected from that inevitable contest in nature between higher and lower forms of life which constitutes disease and leads to premature decay and ruthless destruction.

Bacteriology has, up to now, distributed its favours unequally, but we must not be daunted by this circumstance. It has yielded, in some instances, knowledge of diseases of small, and withheld, in others, knowledge of diseases of great importance. In respect to the common and highly contagious diseases—measles and scarlet fever, for example—progress has been slight. A ray of hope has been cast upon this quest by the announcement[1] that measles can be caused in the monkey by inoculation of infected blood, but this awaits certain confirmation. Similar announcements have been made recently regarding scarlet fever.[2] Since a flood of knowledge has always suddenly flowed from the successful transmission of an obscure disease to the lower animals, these reports have been viewed with eager expectation.

In the case of scarlet fever I fear the expectation is not yet to be realized. We[3] spent last winter in the study of this subject, and failed completely to infect or produce scarlet fever in a wide variety of lower monkeys. Possibly, but not certainly, the higher anthropoid ape, which is still less removed from the human species, is subject to inoculation.[4] The path of success in relation to the refractory diseases is marked by heavy obstacles, but it must be travelled none the less. How often, indeed, has crowning success come to the brave, thoughtful, and adventurous when all but an expiring glimmer of hope had gone! Witness in this connexion the sudden conquest of syphilis, in which the initial victory was won when it was ascertained that anthropoid apes can be infected experimentally. There followed in rapid succession the discovery of the causative spirochaete, the Wassermann clinical test, and the drug salvarsan, the usefulness of which outruns the wide bounds of syphilis itself.

But even after such a victory the drama had not come to an end. The spirochaetal cause could now be discovered regularly where it had been as constantly missed before; doubts and disbeliefs in it were quickly yielding before the rapidly accumulating evidence; but the micro-organism itself resisted all attempts at artificial cultivation. That the spirochaete is a parasite nicely adjusted to living tissues was clear from the difficulties surrounding the experimental inoculation of animals. Now this act also has been played.[5] The *S. pallida* has yielded to artificial culture by Noguchi, and the method sufficing for it has suddenly exposed the whole class of disease-producing spirochaetes, and some innocent species as well, to cultivation and exploitation under laboratory conditions. It is obvious that the more nicely a parasitic organism is adjusted to its host the more difficult it will be to cultivate it outside the host, and the more quickly it will lose in culture its pathogenic power. The *S. pallida*, which for so long resisted the efforts to transmit it artificially to animals and then to cultivate it outside *in vitro*, loses after a few generations, as was to be expected, its disease-producing virulence, while the blood parasites of relapsing and tick fevers in man and spirillosis in fowls, which are less strictly parasitic, and pass a stage of their life in biting insects, retain this power for many generations. In turn, the culture of the *S. pallida* has yielded luetin, which by causing a local allergic or hypersensitive skin reaction has provided clinical medicine with a new means of detecting latent luetic infection.

POLIOMYELITIS.

With this introduction on the more general theme of the hour, I shall invite you to follow with me somewhat minutely the biological investigation of a disease that is still claiming the absorbed attention of both physicians and people—namely, poliomyelitis or infantile paralysis. The disease has just been making the rounds of the world, coming as a very unwelcome intruder to many different countries. Until the present pandemic it was surrounded with mystery and fortified by superstition. It is the story of the working out of the natural history of poliomyelitis, now elucidated in many ways, that I propose to tell you. I have been led to choose this particular disease as my theme both because it has claimed much of my attention during the past several years, and because it illustrates admirably certain general truths to which I desire to call your attention.

Poliomyelitis has been endemic in Northern Europe for many years, but it is only five years since it started on that unique, and as yet unexplained, movement that has carried it around the globe. In America there is no previous history of a general prevalence or epidemic, although local outbreaks of infantile paralysis have from time to time arisen. Some significance attaches to the fact that the first two foci of the present epidemic—I say present, because since 1907 the disease has prevailed severely each summer and autumn at some places in the United States and Canada—arose in the Atlantic Coast cities and in the State of Minnesota in the middle West. The former receive the mass of emigrant population from Europe, and the latter, secondarily, the large contingent of Scandinavian emigrants. The imposition of the infection upon America can thus be accounted for, but no explanation is afforded of the many years of immunity while Scandinavians were constantly arriving, and for the penetration of the disease to other European countries and to far distant parts of the world. However, within the pandemical period the disease has taken on new activity in Norway and Sweden, and as recently as 1911 the latter country has suffered a severe visitation.

On clinical grounds Scandinavian observers[6] had recognized the essentially infectious nature of poliomyelitis, and had followed the evolution of the outbreaks and traced the connexion between many of the cases. They became the defenders of the notion of human carriage, and, by establishing certain unusual clinical forms of the disease—such as the meningeal and abortive—placed this idea on firm ground. The notion was further extended to include healthy carriers of the infection who act as intermediaries between the actively ill and the new victims of infection. These views have all alike been

treated with more or less scepticism by the medical profession; in how far they have come to be supported by later acquisitions of knowledge will appear.

Experimental Transmission.

Apart, then, from these deductions, disputed and disputable, because not supported by certain tests, five years ago the mystery of the disease was wholly unfathomed. The outlook was suddenly brightened when Landsteiner and Popper, in 1909,[7] announced the successful transmission of poliomyelitis to monkeys; but the high hopes raised were as quickly damped by the failure to propagate the experimental disease beyond the first generation. This obstacle was immediately removed when intracerebral was substituted for intraperitoneal inoculation, as was done by Lewis and myself[8] and by Landsteiner and Levaditi.[9] By this means the disease could be, and has been, transmitted through an indefinite number of monkeys. The inoculating matter is, first, the sterile spinal cord of a fatal human case, and afterwards the spinal cord of paralysed monkeys.

The choice of the intracerebral route as superior to the intraperitoneal was not haphazard. All the severe effects of poliomyelitis are inflicted on the nervous system, and upon reflection this fact at once suggested that the parasitic cause of the disease must find favourable conditions for multiplication within the nervous tissues. When the material carrying the germ is put first into the peritoneal cavity it must traverse the blood before it can reach the nervous system, and the blood, as we know, has the power to destroy many forms of germ life. It could, of course, also be reasoned that the specific parasite, in nature, cannot enter the nervous tissues directly, but must use some external route to reach them, and it must, therefore, be capable of surviving outside the brain and spinal cord; and it could be further reasoned that an inoculation into a more accessible part of the body than the brain and spinal cord should be effective, and if effective would bring stronger proof of the actual existence of a parasite in the inoculated matter. This reasoning is unconvincing for two causes: First, the monkey is not naturally subject to poliomyelitis and is, therefore, presumably more difficult to infect at all than is man, so that what may suffice to cause infection in man may fail in the monkey; and, second, it might be possible for pathogenic microbes to reach the central nervous system even in man without entering the blood at all, so that in nature the infectious cause of poliomyelitis might avoid the blood altogether. That this possibility really exists has been proved by experiment as we shall see. Doubtless the first material inoculated into the abdominal cavity carried, besides the living parasites, toxic or other injurious substances that promoted infection in the monkey, but when the nervous tissues of the monkey were similarly injected, being less harmful, the inoculation failed. Bacteriology contains many instances of a similar and, apparently, of paradoxical nature.

The discrepancy has been further elucidated, as will soon appear, but in the meantime it is desirable to inquire whether still other routes of infection do exist for the monkey. Since nervous tissue is favourable for the parasite it was injected into large nerves—such as the sciatic—in order to ascertain whether these furnished a suitable medium of propagation. The parasite grows along the nerve until the spinal cord is reached and produces injury of the cord first at the point of entrance before it extends to and attacks other parts. The injection into the nerve causes no symptoms, but paralysis of the innervated muscles appears after the lapse of a time sufficient for the necessary multiplication of the parasite and its passage into the spinal cord.

Meanwhile, the inoculated monkey shows no other signs of illness, and no other organ is severely affected; the injury is centred upon the nervous tissues. And not only does the parasite grow or flow along the nerve, but it ascends along the spinal cord from lower to higher levels, and eventually reaches the medulla and brain. At last the centres governing respiration are involved, and death by paralysis ensues.

We have now been able to arrive at several important conclusions. The monkey can be made regularly to develop an experimental disease, agreeing in all essential respects with poliomyelitis in man. Inoculation is necessary, since keeping healthy and paralysed monkeys together does not lead to infection. The parasitic cause of the disease can traverse the blood, in the monkey, to reach the central nervous organs, but with difficulty, while it easily traverses the peripheral nerves. That the natural, spontaneous disease, so called, in man, and the induced disease in monkeys are very alike, is further shown by microscopic study of the spinal cord and brain, which exhibit changes that are identical.

Pathological Effects.

The pathological effects are of two kinds: Injury to nerve cells not in the anterior grey matter alone but in the posterior grey matter of the spinal cord and in the intervertebral ganglia, medulla, and brain; and cellular invasion of the pia arachnoidal membrane of the spinal cord and medulla that follow the blood vessels into these parts and pass into the adjacent grey and white matter. The altered vessels permit an escape of albuminous fluid and blood cells into the meshes of the membrane, where they mingle with the cerebro-spinal liquid, and into the spaces in the tissue composing the solid white and grey matter. Sometimes the nerve cells, sometimes the meninges, vessels, and supporting tissues, suffer most. When the nerve cells are extensively injured the paralysis is marked; when the meninges are much affected the symptoms are like those of meningitis. The virus of poliomyelitis displays a high affinity for the nervous tissues, but it is the wide involvement of the nutritive vascular system in the pathological process that subjects the sensitive nerve cells to so high a degree of injury and destruction.

The microscopical conditions we observed in the course of our experiments were suggestive of two things: First, the nature of the parasite itself; and second, the process of generation of the effects or lesions themselves. Up to this time no definite parasite could be detected in the nervous tissues either in human beings or monkeys, nor was anything of the kind found in the blood or other organs. The scarcity of polynuclear leucocytes in the altered cerebro-spinal liquid and spinal cord itself spoke against a simple bacterial parasite. The large number of mononuclear cells spoke rather for a protozoal parasite. Neither could be found, although the most varied methods of staining and cultivation were employed. There remained the possibility of the parasite being invisible or ultramicroscopic and filterable. This it proved to be, for when a portion of the spinal cord of a recently paralysed monkey was made into an emulsion with sterile distilled water or simple saline solution and then centrifugalized to remove the coarse suspended matter and afterwards pressed through a Berkefeld earthenware filter—which excludes ordinary cells, bacteria, and protozoa—the clear liquid resulting was still capable of transmitting the disease. The activity of the filtrate is very great, since a fraction of a cubic centimetre still suffices to cause paralysis and death. The only distinction to be noted between the action of corresponding amounts of the emulsion and filtered fluid is that the former acts more quickly, as would be expected from the fact that it contains a greater number of the invisible organisms. This difference is soon compensated by the multiplication of those in the filtrate, so that the end is the same. By employing somewhat greater quantities of the filtrate for inoculation, the incubation period of the disease can be made the same as that following the use of the emulsion. The disparity is strictly a quantitative one, since the filters retain a part of the minute organisms in their pores and thus reduce the number escaping with the filtrate. The greater the quantity of protein matter present in the fluid the fewer the parasites that pass the filter, and merely because the large protein molecules themselves tend to be held in the pores, and thus render them impervious for the minute organisms. For this reason, also, fluids containing small numbers of the filterable parasites, but still sufficient to cause infection in the crude state, may fail, when filtered, to produce disease, merely because those retained by the filter so far reduce the numbers as to bring them below the surely infecting dose. This reduction sometimes leads to another effect—namely, the slight degree of infection that forms the starting point of active immunization. By building upon such a beginning a high and enduring state of immunity has been achieved.

Filterable Parasites.

The first filterable parasite was discovered by Loeffler fourteen years ago in the fluid lymph obtained from the vesicles of cattle suffering from foot and mouth disease. At the present time eighteen diseases are known that are believed on good ground to be caused by this class of minute living organisms. One alone among them is on the verge of visibility—the parasite causing pleuropneumonia of cattle. It alone has certainly been obtained in artificial culture. The methods of artificial cultivation need still to be worked out; and once they are discovered it is a safe prediction that control over the diseases produced by ultramicroscopic parasites will be quickly increased. The degree of infectivity of certain of the parasites—or viruses as they are also called—is almost fabulous; $\frac{1}{10000}$ c.cm. of a filtered 2½ per cent. suspension of a spinal cord of a paralysed monkey suffices to cause infection and paralysis in another monkey; $\frac{1}{20000}$ c.cm. of infected lymph suffices to produce foot and mouth disease in a healthy calf, and the blood of fowl suffering from chicken plague is still active after being diluted with water 1,000 million times.

Three affections of human beings are contained among the eighteen diseases caused by filterable viruses. They are yellow fever, dengue, and poliomyelitis. With one exception—mosaic disease of tobacco—the remaining fourteen are maladies of domestic animals, and include among them foot and mouth disease, horse sickness, cattle plague, sheep-pox, rabies, vaccinia, hog cholera, and chicken plague. We can at present form no reliable conception of the biology of this class of parasite, although the virus of pleuropneumonia shows affinities with the bacteria, while that of yellow fever, that passes a stage of its existence in mosquitos, probably belongs to the protozoa. It should be remembered that we possess no criterion of their presence other than the power to produce infection. Probably the list of these pathogenic parasites would be increased if methods were known for testing their symbiotic relations or co-operative effects with the usual bacteria and protozoa. Rous's[10] discovery of a filterable agent that causes sarcomatous tumours in the fowl has opened up new fields to exploration. We can make a rough guess as to their sizes, since some pass through thick filters the pores of which are smallest, while others pass the more porous filters with larger interstices only. Were the viruses as large as one-fifth the size of the influenza bacillus they would be beyond visibility with the most powerful optical system of the modern microscope. The dark field microscope and the instrument devised for employing, for photographic purposes, the ultra-violet rays of the spectrum, that has doubled the potential power of the microscope, have failed to bring them into view. On the whole, they resist drying well, and show considerable resistance to disinfecting agents.

The ultramicroscopic viruses employ no single means of effecting entrance into the body. Some utilize insects to inject them into the blood. Mosquitos inoculate the parasites of yellow fever and of dengue in man and among animals the virus of horse sickness; while flies inject the virus of parrot fever, and worms, and other insects, through mere contact with infected and then with uninfected tobacco plants, disseminate the parasite of mosaic disease. The viruses of rabies, vaccinia, and fowl-pox, gain entrance through skin wounds; those of hog cholera, foot and mouth disease, and chicken plague, by swallowing, while the parasites of variola and of pleuropneumonia are inhaled with air. These are the main channels, but not the sole routes of infection, since viruses that ordinarily enter the body by the respiratory mucous membrane may occasionally enter through a skin abrasion, etc.

Immunity.

It is significant that upon recovery from this class of infections a high and enduring degree of immunity is left behind. We have no knowledge of toxic substances, in the common sense, being produced by the filterable viruses, and therefore know nothing of the formation of antitoxins or bodies that neutralize poisons. The principles upon which the immunity depends appear to be chiefly microbicidal or substances that act directly upon the living parasites and destroy them. In some instances it has been possible to produce an actively immune state without at the same time causing severe disease by employing for inoculation modified and weakened viruses, and viruses combined with immune serums carrying the corresponding microbicidal substances. Once a certain active immunity is obtained it can be heightened by repeated injections of more active materials until a high degree is achieved. In the same manner immune animals that have recovered from disease are capable of having this immunity reinforced by subsequent injections of the active virus. Blood taken from the immune animals has been employed in practice in two ways: to protect for a brief period exposed animals from acquiring infection, and to bring about an actively immune state through inoculation with adjusted mixtures of virus and corresponding immune serum. The injection of viruses into animals not themselves subject to infection has, in a few instances, yielded immune serums. In this way a serum for foot and mouth disease has been prepared in the horse. Speaking generally, homologous serums are more active than heterologous, or, in other words, an immune cattle serum will act better in cattle than will immune horse serum; but curative serums in a real sense have not yet been produced for this class of diseases.

It is of great interest to determine the correspondence between the general data I have just reviewed and the special facts of poliomyelitis which have been shown to arise in consequence of an invasion of the nervous tissue by an ultramicroscopic or filterable virus. We may proceed to check off rapidly the main facts: The virus stands midway in point of size between the finest and coarsest examples. It passes readily through the more coarse and slightly through the finest filters. It is highly resistant to drying, light, and chemical action. In dust, especially within protein matter, it survives weeks and months; in diffuse daylight indefinitely, and resists the action of pure glycerine and carbolic acid in one-half per cent. solution for many months. When animal tissues containing the virus suffer softening and disintegration or disorganization by mould, the virus survives. Recovery from poliomyelitis in man and monkey is attended and produced by an immunization of the body. During this process microbicidal substances appear in the blood that are capable of neutralizing the active virus. This acquired immunity has, in the monkey, been reinforced by subsequent injection of large quantities of the living virus. Active immunity can be achieved by first injecting minute and later large amounts of the virus, and an adjusted mixture of immune serum and active virus will confer a beginning low active immunity capable of being heightened. Certain alien large animals, among which the horse and sheep are especially worth mentioning, are subject to immunization through injections of emulsions of the spinal cord and brain of paralysed monkeys, and can thus be made to yield serums possessing microbicidal power, and capable of conferring, as do human and monkey immune serums, a degree of passive immunity. Thus far no immunizing effect has been accomplished with the dead virus. Unless some growth and multiplication take place no immunity arises.

The Mode of Infection.

These facts show a close correspondence between the properties of the virus of poliomyelitis and those of the ultramicroscopic organisms in general. There remains to be considered the data bearing upon the manner of entrance of the poliomyelitic virus into the body, or, in other words, upon the mode of infection. Analogy with other diseases produced by filterable viruses excludes no one of the possible modes, since their manner of entrance is widely varied, as we have seen. This question is of the utmost importance, since with all diseases prevention is far better than the most perfect cure, and for poliomyelitis there exists at present no specific or true curative treatment. Moreover, for the most part, when the disease is first recognized, it has already caused irreparable damage, and, though the more general examination of the spinal fluid obtained by means of lumbar puncture for purposes of diagnosis may possibly lead to a much earlier recognition of the disease, yet its prevention will always remain the result to be aimed at. It is quite certain that an understanding of the mode of infection would lead inevitably to the framing of measures of prevention that with reasonable certainty could be expected to exercise control over the epidemic spread.

Two answers may be returned to the question—one based upon observation of human cases of poliomyelitis, and the other based upon experimental tests arranged to elicit specific replies. The first answer cannot achieve anything higher than strong probability; the second, to be valid, must explain the phenomena attending the human infection, as well as those of the experimental disease.

We are asked to account for certain data, of which the following is a brief statement. Epidemic poliomyelitis is pre-eminently a disease of early childhood, and finds the highest percentage of its victims in the first five years of life, but does not wholly spare older children or even adults. It is admittedly infectious; and while it is true that many more instances of single than of multiple cases occur, yet multiple ones are not by any means rare. The prevailing views on this topic are being modified rapidly by the recognition of the abortive and ambulant examples of the disease. The period of greatest prevalence is during the months of August, September, and October in the northern hemisphere, and the corresponding months in the southern hemisphere, but the epidemic begins in the early spring and summer months, and the disease does not wholly disappear during the winter months. It does not, therefore, necessarily die out at any period of the year. In endeavouring to trace the avenue of entrance of the virus into the body certain facts regarding its distribution in the body should be recapitulated and considered.

The infectious agent of poliomyelitis attacks chiefly the central nervous system. Indeed, it has been detected regularly in the spinal cord and brain, and in the mesenteric lymph nodes among all the internal organs. It has also been detected in the mucous membrane of the nose and throat, and in the mucous secretions of this membrane, and in the mucous secretions of the stomach and the small and large intestine. The virus has not been detected in such important organs as the spleen, kidneys, liver, or bone marrow. The fact is significant, but in attempting to interpret it account should be taken of the circumstance that at present we possess one means only of detecting the virus, and that is its transmission to monkeys, in which it produces characteristic paralyses and anatomical changes. On this account small quantities of the virus may conceivably escape discovery. However, the conclusion is none the less inevitable that detectable amounts of the poliomyelitic virus exist only in the few situations and organs mentioned. The distribution of the virus is identical in human beings the subjects of the so-called spontaneous poliomyelitis and in monkeys in which the experimental affection is produced. Nor does it matter how the experimental inoculation is accomplished, and whether the virus is introduced by injection into the brain or large nerves, or subcutaneous tissue or peritoneum, or whether it is merely applied to the nasal mucous membrane, which, it should be emphasized, next to direct intracerebral injection, affords the surest means of causing the experimental disease. In whatever way the infection is produced purposely, the distribution of the virus in infected monkeys is the same as in infected human beings.

The virus is one that is not known to increase aside from the infected body, and hence, in order that it shall be capable of propagating poliomyelitis, it must secure a means of escape from the infected animal. The escape is now known to occur along with the secretions of the nose and throat, and the discharges from the intestine. We are obliged, therefore, to ask ourselves what the means are by which the virus confined within the interior reaches these external surfaces of the body.

Let us begin by disregarding for the moment the essential point of the way in which the virus probably enters the body in infected human beings, and give our attention to the way in which it escapes in the infected monkey into the nose, throat, and intestines. We may first consider the instance in which the virus is deposited in the brain, in which it becomes sealed, as it were, and cut off apparently from the exterior of the body. Having been injected into the brain, the infectious micro-organism constituting the virus multiplies both within and about the brain tissue at the site of inoculation. As multiplication progresses, the virus leaves the original site of injection and wanders through adjacent and distant parts of the central nervous tissues, becoming implanted in the medulla, the spinal cord, and the intervertebral ganglia, as well as reaching the pia arachnoidal membranes, or meninges, in which it also spreads. Ultimately, when the virus becomes sufficient in amount, it brings about anatomical changes in the nervous system, one of the results of which is paralysis. The period intervening between the inoculation and the appearance of paralytic symptoms may be as brief as two or three days, or as long as three, four, or five weeks. The great disparity in this period depends upon the amount and quality of the virus, as well as the degree of resistance of the inoculated monkey.

The virus, which has found its way to the meninges, does not long remain in the cerebro-spinal fluid, with which it escapes in part into the blood, where it does not appear to undergo any further increase in amount, and indeed seems even incapable of surviving for long. A part also of the virus contained within the cerebral fluid escapes regularly by way of the lymphatic channels surrounding the short nerves of smell that pass from the olfactory lobes of the brain to the mucous membrane of the nose. It has long been known that there is an intimate connexion between the lymphatic vessels of the nasal mucous membrane and the lymphatic spaces of the pia arachnoidal membrane. The virus, once having gained the mucous membrane of the nose, may even escape into the mucous secretion, with which it is carried into the mouth and in part swallowed, or it may become established in the substance of the nasal membrane, where it undergoes subsequent multiplication and increase. As a matter of fact, both these things occur. The virus escapes with the secretions partly externally to the infected body, and a part of it is swallowed with the secretions themselves, while a persistent infection of the secretions is maintained by means of the increase that takes place in the membrane itself. In this way is assured the escape of the virus directly into external nature, as well as the contamination of the gastro-intestinal cavity, with the discharges of which it becomes commingled. Once implanted upon the intestine, multiplication not improbably continues for a time, and another source of invasion of the body is thus afforded the parasite. From the intestine it reaches in some amount the mesenteric lymph nodes, and thus enables us to account for the occurrence of the virus in those lymphatic nodes, which thus form a notable exception to the general internal organs of the body.

We have now followed the route by which the poliomyelitic virus implanted within the apparently closed cavity of the skull reaches the exterior of the body. It is obvious that in the spontaneous form of the infection in man no such mode of introduction of the virus can occur. The virus must, indeed, enter the human body by some external channel, after which it seeks and becomes implanted upon the central nervous system. It is known that in monkeys the virus is incapable of passing the barrier of the unbroken or slightly abraded skin, of being taken up from the stomach or intestine unless the functions of these organs are previously disturbed and arrested by opium, and it is further known that it traverses with difficulty or even not at all the substance of the lungs. On the other hand, it is established that the virus passes with readiness and constancy from the intact or practically intact mucous membrane of the nose to the central nervous system.

To illustrate this point I wish to describe briefly an experiment. The spinal cord of a paralysed monkey always contains the virus we are considering. If a camel's hair pencil or pledget of cotton is covered with some of the broken-up tissue of such a cord and painted upon the mucous membrane of rhesus monkeys, these animals will develop in due time the paralysis and other symptoms of poliomyelitis. Hence the virus enters the body from this surface even though no gross injury has been inflicted upon the membrane.

The Olfactory Nerves.

We should now ask ourselves if the virus actually ascends to the brain by the direct path of the olfactory nerves or indirectly after first entering the blood. This is the same question that has been buffeted about in regard to epidemic meningitis. The meningococcus is found in the nasal mucus of persons in contact with cases of meningitis, and in the sick themselves. It is not dis-

puted that the meningococci settle on this membrane, but opinion is divided as to whether it goes at once to the membranes of the brain or first penetrates into the blood. To produce meningitis in monkeys it does not suffice to inoculate the nasal membrane; the meningococci must be injected into the meninges themselves. But so inoculated they escape in part along the nerves of smell into the nose. The virus of poliomyelitis is so active that implantation in the nose does suffice to cause infection. If a monkey is sacrificed about forty-eight hours after an intranasal inoculation and the brain and spinal cord are removed, and then the olfactory lobes, portions of the medulla and spinal cord are separately inoculated into other monkeys, infection is produced by the olfactory lobes alone, since in this brief period the virus has not reached other and more distant parts of the nervous organs. Were the virus distributed by the blood, the medulla and spinal cord would have become infective, rather than the olfactory lobes, since they exhibit a greater selective affinity for the parasite. The conclusion is unavoidable that the virus ascends by the nerves of smell to the brain, multiplies first in and about the olfactory lobes, and, in time, passes, as I believe, into the cerebro-spinal liquid, which carries it to all parts of the nervous organs. We have already learnt that the virus can pass along a large nerve, such as the sciatic, which carries it first to the lumbar cord, whence it ascends to higher levels. We need not, therefore, be astonished to find that it can wander along the olfactory nerves and then descend to lower levels. The large peripheral nerves are prevented anatomically from becoming infected in nature, while the small olfactory filaments are advantageously placed to act as the means of transportation.

Hence the view I desire to place before you: That the nasal mucous membrane is the site both of ingress and egress of the virus of poliomyelitis in men. Support for this view is found also in the study of the microscopic changes in the meninges and the central nervous tissues. Since the virus survives in the dried state it may be carried in dust; and in one instance it has been detected in sweepings from the room occupied by a person ill with poliomyelitis.[11] Its distribution as spray in coughing and speaking is readily accomplished, and by this means both active cases and passive carriers may conceivably be produced. Still, one link in the chain of causation of poliomyelitis as here outlined remained to be forged. The clinical evidence is strong in suggestion that human carriers of the poliomyelitic virus exist. The virus has now been detected in the secretions of the nose, throat, and intestine of persons suffering from abortive or ambulant attacks of poliomyelitis.[12] The unrecognized examples of the abortive disease play a highly important part in the dissemination of the virus, through which the area of infection is extended, and the number of the attacked increased. A similar part has been accorded by clinical observation to the healthy virus carrier, and the healthy carrier is the last to be detected, and his existence confirmed experimentally. The obstacles in the way of this confirmation are considerable, but not insuperable. It is to be remembered that we possess no means of discovering the virus except that of animal inoculation. Should the experimental results arising from the inoculation of the secretions of the nose and throat of such healthy carriers be confirmed, the evidence for the mode of infection as here outlined would be complete. The membrane of the nose and throat is far more vulnerable in young individuals, whence arises the greater prevalence during childhood of those diseases the causes of which seek this avenue of entrance into the body. Among them are included diphtheria, measles, scarlet fever, and meningitis.

Would the establishment of the respiratory avenue of entrance of the virus exclude all other modes of possible infection? By no means. Plague bacilli are known to be inoculated into man by rat-fleas; but the pneumonic form of the plague is admittedly caused by respiratory inoculation. Diphtheria arises upon the mucous membrane of the throat, but can develop in a wound of the skin; the virus of small-pox enters by way of the throat and nose, but can enter by a skin abrasion; the virus of foot and mouth disease is taken in with food, but produces infection when injected into the skin. Hence, at the moment while knowledge is still recent and not yet perfect, the too absolute adherence to one point of view is to be avoided.

Possible Insect Carriers.

Indeed, the preponderance of cases in the late summer and autumn months early suggested an insect carrier of the infection. House-flies can act as passive contaminators, since the virus survives upon the body and within the gullet of these insects. It has not proved possible thus far to infect the common varieties of mosquito and the body and head louse, while success has resulted in one instance in producing infection in bed-bugs, which were made to feed upon the blood of inoculated monkeys. The virus remained alive within these insects for a period of many days. The inoculation of monkeys with a filtrate prepared from them gave rise to characteristic paralysis and anatomical lesions. This result is significant, since it shows that insects are capable of taking up the virus from the blood where it exists in minimal quantities, and in harbouring it for a considerable period in an active state; but it does not show that multiplication occurs within them, or that in nature they act as the agents of inoculation. A tentative announcement has been made recently by Rosenau[13] that the stable fly (*Stomoxys calcitrans*) can take up the virus from the blood of infected monkeys and reinoculate it into healthy ones, which will become paralysed. The experiment awaits confirmation, and, after confirmation, convincing application to the circumstances surrounding infection in human cases of poliomyelitis.

Domestic Animals.

The frequent prevalence of epidemics in sparsely populated country districts has led, moreover, to consideration of domestic animals as sources of the infection. Paralysis of dogs, horses, pigs, and fowls, has been observed not uncommonly, but thus far without clear correlation with paralysis in man. Perhaps the most frequently observed coincidental paralytic diseases have been between hens and human beings. Undoubtedly since the wide prevalence of epidemic poliomyelitis, the existence of a paralytic disease among barnyard fowls has been more commonly noted. Possibly the condition has not actually become more frequent, but owing to the circumstance mentioned it has been oftener observed. It appears that the paralysis among fowls is caused not by lesions of the central nervous system, but by lesions of the peripheral nerves, and is due to a peripheral neuritis. It has not been found possible to transmit by direct inoculation the paralytic disease from chicken to chicken, or from chicken to monkey, or from paralytic monkey to chicken. However, it has been found possible to develop the paralysis in the laboratory by keeping the chickens in confinement for some time, and by supplying them an unusual and improper form of food. It has proved as little possible to transfer the paralytic affection of dogs from one individual to another by direct inoculation, or from dog to monkey, or from paralysed monkey to dog, or to set up paralysis in monkeys by inoculating them with nervous tissue obtained from paralysed pigs, or to produce paralysis in pigs with the virus of paralysed monkeys. These failures do not, of course, exclude the possibility that a reservoir for the virus may exist among domesticated animals that do not even respond to its presence by developing paralysis or other conditions which could be recognized as resembling poliomyelitis in man.

The manner of action of the virus of poliomyelitis in rabbits provides an illustration which shows how necessary it is to avoid general deductions in this field. At first it was strenuously denied that rabbits could be infected at all with the virus of poliomyelitis, and the examples of supposed successful inoculation reported were entirely disbelieved; but it must now be accepted that young rabbits occasionally, but by no means generally, are subject to inoculation with the virus of poliomyelitis, at least after it has passed through a long series of monkeys. Apparently a small percentage only of the inoculated rabbits develop any obvious symptoms, and these die, as a rule, during convulsive seizures which come on suddenly. A given virus has up to the present been sent through a series of six rabbits, after which it has failed to be further propagated. From the sixth series it has been reimplanted on the monkey, in which animal typical paralysis has been produced. It remains to add that the rabbits which succumb to the inoculation do not show any characteristic alterations of the central nervous system or other organs, as far as has been

determined. The monkey, on the other hand, invariably shows the typical lesion of the central nervous system.

Sporadic Cases.

Long before epidemic poliomyelitis had the wide distribution or claimed the attention now accorded it, instances of infantile paralysis were known to every one. Almost every community could point to one or more examples of the condition, and no one entertained the suspicion that the cause of the paralysis was an infectious or even contagious disease. Are these isolated cases of paralysis occurring among infants of the same nature as the epidemic paralysis, or has there merely been a confusion of names? We possess means that permit an answer to this important question. Recovery, as you recall, is associated with enduring immunity, and the person or animal immune to poliomyelitis carries in his blood principles that neutralize the virus causing the disease. The blood of normal persons or animals lacks this property in any real degree. The test is, therefore, easily made: a mixture of the serum of the blood and virus are prepared, and after being in contact for a time, is injected into a monkey. Thus it has been determined that the two diseases are caused by the same parasite, and it has been found that the neutralizing principles are still present as long as twenty-five years after the attack of paralysis, and doubtless persist through life. This test has been employed likewise to identify abortive cases of poliomyelitis in which paralysis has not appeared at all.

There is nothing unique in this apparently paradoxical situation. Most, if not all, the epidemic diseases prevail at some time as sporadic affections—that is, as diseases of occasional occurrences. This is true of influenza, plague, and particularly of meningitis, with which poliomyelitis displays so many affinities. Knowledge is still very imperfect as to just what happens when an epidemic spread of a sporadic disease takes place. Sometimes conditions arise that favour rapid transference of the infecting microbe from individual to individual through which a rise in virulence is accomplished, very much as is done every day in the laboratory to enhance the potency of cultures. In respect to poliomyelitis, as seems also to be the case with meningitis, a fresh importation of an already enhanced virus probably occurs and is the immediate cause of the epidemic. The introduction may be at one point or at several points simultaneously, according to which the epidemic arises in and spreads from a single centre or from many foci. Finally, sports or abnormally virulent parasites appear, prevail actively for a period, and then become reduced to an average degree of intensity, perhaps never to rise again. Some of the exceptionally severe epidemics of which history tells us may be thus accounted for. Such sports have been encountered in laboratories in regard to both pathogenic bacteria and protozoa.

Strains of Varying Virulence.

Are biologically different strains of a poliomyelitic virus known? The evidence at hand is to the effect that different strains or races certainly exist if virulence be taken as the measure. German, Austrian, and French pathologists found that of the human specimens of spinal cords submitted to them for study, about one-half could be inoculated successfully into monkeys, and less than this number could be propagated through successive animals. In America all the original specimens were successfully inoculated, but certain samples were far less active than others. At the beginning many of the inoculated monkeys survived the infection, sometimes with, sometimes without, enduring paralysis of leg or arm. Later, fewer survived, and after many passages of the virus from monkey to monkey, all became infected and all succumbed. The Swedish virus of 1911 appears to be the most powerful yet studied.[13] This is indicated by the fact that saline washings of the nose and throat and intestine could be inoculated successfully, after removal of all bacteria through filtration, in nearly every instance. In America it has been difficult to procure infection with these materials, from which it has been concluded that the virus displays degrees of infectiousness for monkeys. There are reasons for supposing that similar variations exist for man himself.

We may not, and probably shall not, know certainly whether this variability is restricted to the quality of virulence or whether true types or races of the virus exist until artificial cultivation has been accomplished. Bacteriology has been singularly enriched recently by discoveries relating to biological types of certain microbes, and practical medicine is destined to benefit largely by the strong light which they have thrown upon perplexing questions of specific therapeutics. I am tempted to lead you aside a little way into this subject, just because it is so full of suggestion and promise, and not merely with promise, since the fruits of discovery are being already tasted.

The Pneumococcus: a Digression.

The pneumococcus causes many kinds of inflammation and one typical disease that prevails everywhere—namely, acute lobar pneumonia. Not infrequently there attends the pneumonia, and sometimes there appears independently, such inflammations as peritonitis, pleuritis, and meningitis, caused also by the pneumococcus. Now pneumococci possess in common biological features regarded usually as sufficient to distinguish them—namely, form, staining properties, growth, virulence, and solubility in bile salts. But they have another quality that serves to distinguish them more finely, revealing different types among apparently similar organisms. By testing pneumococci from many different sources against an immune serum prepared with a single kind of the coccus, it has been found that the cocci are not all alike, but that a predominant type and several subsidiary types occur in nature.[14] Such a serum prepared with a given type of pneumococcus is neutralizing alone for that one, and for no other one. The clinical reports on the antipneumococcus serum employed as a curative agent are contradictory, and one cause for this is now apparent.

Pneumococcus meningitis can be produced in monkeys by injecting subdurally, by lumbar puncture, a virulent culture of pneumococcus; it is invariably fatal. Antipneumococcus serum alone injected subdurally can change the outcome very little. But this infection is subject to combined chemo-therapy and serum-therapy, in which the chemical agent consists of sodium oleate that alone attacks and dissolves the pneumococcus. Acting separately in the body, sodium oleate can accomplish little; it requires the assistance of the immunity principles. Acting together, the two agents quickly bring the infection under control, and recovery follows. This happens even after the pneumococci have entered the blood stream and begun to multiply there. The effects of the soap and serum compound are, however, restricted to the type of pneumococcus represented by the immune serum in the mixture.[15] When the type of micro-organism and serum differ absolutely no therapeutic action follows. This obstacle to the practical employment of this method of specific treatment will doubtless be reduced or even wholly set aside by preparing a true polyvalent immune serum that will represent not many cultures of the pneumococcus taken at random, but the several types or races occurring in nature. We already know the number to be few.

It has become the custom to speak of these types of microbes as resistant or "fast," but the term is relative merely. The fact and degree of fastness will be revealed by the source of the test serum. But within a given microbic species this quality of resistance may well appear against chemical bodies as well. Pneumococci, for example, vary in properties by gradual gradations in the direction of the streptococcus, which besides differing in still other biological properties chances not to dissolve in bile. The gradients of pneumococci approaching the streptococcus are progressively less acted upon by sodium oleate. The trypanosome of sleeping sickness is less subject to the therapeutic action of certain organic arsenic compounds in some regions in Africa than in others. The antimeningitis serum suppresses the growth and multiplication of most meningococci, but not of all. This quality of fastness is not alone innate, but can be developed artificially as a mutation both against serum principles and chemical drugs, and may persist. Infectious diseases showing a strong tendency to relapse in course of recovery are caused by microbes tending to flourish as races or types. Relapsing fevers that pass through three or four exacerbations on the way to recovery are attributed to spirochaetes assuming a corresponding number of distinct forms. Infections tending to many relapses, of

which lues is an example, are attributed to parasites capable of flourishing in many such types, of which one part is innate and the other the result of mutations under the influence of curative serum or drug. Fortunately there appears to be no parasite capable of performing indefinite mutations; and experience is teaching that the more precise, specific, and vigorous the means employed to control infection, the smaller the risk of mutation and the greater the probability of suppression of the parasitic agent of disease.

Specific Treatment of Poliomyelitis.

In 1886[16] Theobald Smith first clearly pointed out that the injection of dead bacteria confers active immunity to subsequent inoculation with virulent materials. Now the employment of dead bacteria is widespread, both for preventing and for healing disease. Wright especially is to be credited with the general application of the method to therapeutics. While the limits of value of inoculation, as it is termed, are not yet defined, and it promises, theoretically, more for the subacute and chronic than for the acute infections, I am inclined to the belief that to be really effective attention will need more and more to be accorded to the question of specific type in the infecting bacteria.

In pursuing the devious courses of infection, of which examples have just been given, the fact has emerged that the effectiveness of curative means will be determined not only by the intrinsic qualities of the parasites, but also in a high degree by the manner of location and distribution of the parasites themselves within the infected host. Whether they have a general distribution throughout the blood and tissues, or whether they are confined within an important organ or part, may be the factor determining the ease with which they can be reached, not only by the natural curative principles of the body, but also by artificial curative agents introduced into the body.[17]

The parasite, struggling to survive, withdraws at one time into situations to which the curative substances gain access imperfectly and with difficulty, causing thereby local infections more or less cut off from the general circulation and the curative substances purveyed by the blood. This is the condition met with in focalized inflammation and in infections of specialized portions of the body, such as the great serous cavities, that receive a modified and dilute lymph secretion carrying reduced quantities of the protective principles contained within the blood. The quality of lymph in the several serous cavities and in the various tissues is not the same, and the lowest limit of strength is reached by the cerebro-spinal fluid that functions as the lymph of the brain and spinal cord. The exclusion of dissolved substances from the cerebro-spinal liquid is a provision of great importance but is not an unmixed good, for while it affords protection to the sensitive nervous tissues from injurious chemicals, it deprives them also of curative principles. Happily this deficiency has now been superseded by a method of direct local treatment by injections that has given excellent results in meningitis, and is now being employed in luetic affections of the meninges and central nervous organs with encouraging results.[18]

Remote as some of them may seem, the considerations to which I have called your attention have a bearing, more or less vital, upon the problem of a specific and effective treatment of poliomyelitis. Poliomyelitis is not a disease with a very high mortality; its chief terror lies in its appalling power to produce deformities. When death does occur, it is not the result, as in many infections, of a process of poisoning that robs the patient of strength and consciousness before its imminence, but is caused solely by paralysis of the respiratory function, sometimes with merciful suddenness, but often with painful slowness, without in any degree obscuring the consciousness of the suffocating victim until just before the end is reached. No more terrible tragedy can be witnessed.

I have already laid before you certain facts regarding immunity in poliomyelitis, and it remains to be added that the employment for treatment of the immune serum, taken from monkeys or from human beings, exercises a definite, if not very strong, protective action upon inoculated monkeys. Either the disease is prevented altogether or its evolution is modified in such a manner as to diminish its severity. When the virus used for inoculation is highly adapted to the monkey and thus very virulent, it is more difficult to control the result than when it departs less from the original human type, and is less active.

The immune serum has thus far acted best when it was injected into the subdural space on several successive days. This is in conformity with the fact that however introduced into the body the virus establishes itself in communication with the cerebro-spinal liquid where it propagates for a time. After a time the virus localizes in the nervous tissue itself and becomes accessible not only from this liquid but, probably, from the general blood also. The serum introduced into the subdural space soon escapes into the blood, and thus a double action is secured; on the one hand, it reaches the nervous tissue directly from the cerebro-spinal liquid, and on the other hand with the blood. An immune horse serum at first gave disappointing results, but latterly its employment by intramuscular injection has given more promise. But none of the serums mentioned can be regarded as having more than touched the fringe of the problem of a cure for the disease.

Such brilliant success has been recently recorded in respect to the specific chemical therapeutics of infection that an effort has been and still is being made to attack the problem from this quarter. Here also only a starting point has been secured, and the subject merely opened to further experimentation. The point of departure which we have adopted is the drug hexamethylenamin (urotropin), which possesses a degree of antiseptic action in the body and is known to be secreted into the cerebro-spinal liquid. When the drug is administered by the mouth it can be detected by chemical tests in the liquid in a short time. When inoculation of virus and administration of the drug are begun together, and the administration continued for some days afterwards, the development of the paralysis is sometimes, but not always, averted. Hexamethylenamin lends itself to modifications by the addition of still other antiseptic groups to its molecule. We have tested a large number of such modifications, and have found certain ones to exceed the original compound in protective power, and others to promote the onset of paralysis. This is the common story of drugs. None are wholly without some degree of injurious action upon the sensitive and vital organs of the body. But manipulative skill has already succeeded in eliminating the objectionable and improving the valuable features of drugs so that they exert their action but little upon the organs and severely upon the parasites, when they become useful as therapeutic agents. This process may be called sundering the organotropic and parasitotropic effects. Whether this can be successfully accomplished with this class of compounds cannot be predicted. But if not, the quest will be transferred to still other drugs. When it is accomplished, the victory will be won. By whom will the victory be won, and when? Ours is the office of story-teller and not the vision of the prophet.

CONCLUDING TRIBUTE TO HUXLEY.

In giving Huxley to science the Charing Cross School of Medicine conferred a great benefit upon the world. In imbuing him with the ideals of biological science it performed an especial service for America. For, in 1876, Huxley journeyed to Baltimore to deliver the address at the formal opening of the Johns Hopkins University, at which time he outlined in essence the plan of medical education which, twenty years later, was adopted and put into practice at the Johns Hopkins Medical School. The example of this wise foundation, inspired by Huxley, has acted far and wide throughout the United States as a regenerating force upon medical education.

REFERENCES.

[1] Anderson and Goldberger, *Bulletin of the U.S. Public Health and Marine Hospital Service* 1911, No. 62. [2] Cantacuzène, *Comptes rendus de la Société de Biologie*, 1911, lxx, 403; Bernhardt, *Deutsche medizinische Wochenschrift*, 1911, xxxvii, 791, 1062; *Centralblatt für Bakteriologie, Parasitenkunde, und Infectionskrankheiten*, Abteilung I, Referrte, Supplement. 1911. 1, 27. [3] Draper, George, unpublished studies. [4] Landsteiner, Levaditi, Prasek, *Annales de l'Institut Pasteur*, 1911, xxv, 754. [5] Noguchi, *Journal of Experimental Medicine*, 1911, xiv, 99; 1912, xv, 90; 1911, xiv, 557; 1912, xvi, 199. [6] Wickman, *Beiträge zur Kenntnis der Heine-medizinschen Krankheit*, Berlin, 1907. [7] Landsteiner and Popper, *Zeitschrift für Immunitätaforschung*, original, 1909, ii, 377. [8] Flexner and Lewis, *Journal of the American Medical Association*, 1909, liii, 1639; *Journal of Experimental Medicine*, 1910, xii, 227; Flexner, *Journal of the American Medical Association*, 1910, lv, 1105; Flexner and Clark, *Journal of the American Medical Association*, 1911, lvii, 1685. [9] Landsteiner and Levaditi, *Comptes rendus de la*

Société de Biologie, 1909, lxvii, 592; for general bibliography consult Römer, *Die epidemische Kinderlähmung*, Berlin, 1911. [10] Rous, *Journal of the American Medical Association*, 1911, lvi, 198. [11] Neustaedter and Thro, *New York Medical Journal*, 1911. [12] Kling, Wernstedt, and Pettersson, *Zeitschrift für Immunitätsforschung u. exper. Therapie*, original, 1912, xii, 316. [13] Rosenau, M. J., communication at the International Congress of Hygiene and Demography, Washington, 1912. [14] Neufeld and Händel, *Zeitschrift für Immunitätsforschung*, original, 1909, iii, 159; *Arbeiten aus dem kaiserlichen Gesundheitsamte*, 1910, xxxiv, 293; *Berlinerklinische Wochenschrift*, 1912, xlix, 480. [15] Lamar, R. V., *Journal of Experimental Medicine*, 1911, xiii, 1; 1912, xvi, 581. [16] Salmon, D. E., and Smith, Theobald, *Proceedings of the Biological Society of Washington*, 1884–6, iii, 29. [17] Flexner, *Boston Medical and Surgical Journal*, 1911, clxv, 709; the Harben Lectures, *Journal of State Medicine*, 1912, Nos. 3, 4, and 5. [18] Swift and Ellis, *New York Medical Journal*, 1912, xcvi, 53; Wechselmann, *Deutsche medizinische Wochenschrift*, 1912, xxxviii, 1446.

The Bradshaw Lecture

ON

THE DIAGNOSIS AND TREATMENT OF INCIPIENT PULMONARY TUBERCULOSIS.

DELIVERED AT THE ROYAL COLLEGE OF PHYSICIANS, LONDON, ON NOVEMBER 5TH, 1912.

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(Abstract.)

WILLIAM WOOD BRADSHAW, M.D., F.R.C.S., M.R.C.P., in memory of whom this lectureship was established by his widow, was a Member of this College engaged in general practice. It seems appropriate, therefore, that the Bradshaw lecturer should select a subject in which general practitioners are specially interested, and should endeavour to elucidate questions of diagnosis and of treatment in such a manner as shall be helpful to them in their daily work. No subject is more important to the general practitioner than the diagnosis and treatment of incipient pulmonary tuberculosis; no morbid condition more frequently claims his attention; in none is a correct diagnosis and a suitable treatment more necessary for the welfare of the patient and for the reputation of the practitioner. . . .

At this time, when a national effort is being organized, and when large contributions from the national resources are to be furnished in support of this effort, it is highly desirable that the most effective measures and, as far as possible, the least costly should be adopted, so that the greatest possible benefit may be secured, and the funds provided may be most economically administered. This unexpected but most welcome therapeutic opportunity is a call to the medical profession to consider carefully what measures they will recommend. It is ours to direct this campaign. Are we quite clear in our minds as to the precise measures which ought to be employed?

[The lecturer then briefly reviewed the statistical evidence as to the prevalence of tuberculosis, and arrived at the conclusion that about 50,000 deaths from pulmonary tuberculosis occurred every year in the United Kingdom, and 20,000 more from other forms of tuberculosis.

After pointing out that the pathological, clinical, bacteriological, and experimental work of the last few years had shown that the problem of tuberculosis might be resolved into two separate problems of unequal magnitude arising from the action of two distinct types of the *Bacillus tuberculosis*—the "bovine" and the "human"—he went on to say that the bovine type was found chiefly in tuberculosis of the abdominal organs and of the cervical and bronchial glands, and even in these almost exclusively in early childhood. It appeared, he said, to have very little influence in the production of pulmonary tuberculosis, and as five-sevenths of the total mortality from tuberculosis was due to disease of the lungs, it was quite clear that the complete annihilation of the bovine type of the bacillus would make very little difference to the mortality from phthisis. The crucial point of the struggle against tuberculosis in general was whether it was possible to abolish phthisis. If pulmonary tuberculosis could be annihilated, there would be no spread of infection by means of sputum, and there would be little or no infection of other organs in the patients themselves. For the answer, he said, we must go far back to the individual case of incipient pulmonary tuberculosis and to the individual medical practitioner who is called upon to diagnose and to treat it. The abolition of pulmonary tuberculosis is possible on two conditions only: first, that every practitioner shall learn how to detect the disease at its first appearance, long before any bacteriological evidence is available; and, secondly, that a method of treatment can be employed by the practitioner, in the patient's own home, which will be simple, harmless, completely effective, and yet inexpensive.]

I. THE DIAGNOSIS OF INCIPIENT PULMONARY TUBERCULOSIS.

At present the great majority of general practitioners do not feel justified in giving a definite diagnosis of incipient pulmonary tuberculosis until the bacillus has been found in the patient's sputum, either by themselves or by some acknowledged bacteriological expert. This is unfortunate, for the disease is often present for weeks or months, and may have spread extensively in the lungs before a positive bacteriological report can be given. A negative bacteriological report may be entirely misleading and, indeed, calamitous in its results, for it may delude the patient and the practitioner into a false sense of security, so that the disease goes on its way unchecked until its manifestations become too plain to be any longer ignored. The fact that no bacilli can be detected in the sputum is no proof whatever that they do not exist in the lungs.

Value of Percussion.

The most recent monographs and articles in the medical journals all, with one consent, in considering the diagnostic evidence of incipient pulmonary tuberculosis, lay stress on auscultation and say very little about percussion. Yet the auscultatory signs may be very slight indeed when the percussion signs are well marked and entirely distinctive. Quite large dull areas may be present in the positions characteristic of a tuberculous infection of the lungs, and yet there may be few or no evidences of local catarrh; there may be nothing abnormal observable by the most careful auscultation, except a defect in the entry of air.

In incipient pulmonary tuberculosis the earliest signs are local areas of dullness with defective air entry as the only constant auscultatory phenomenon, though some catarrhal sounds may possibly be present in addition. The tuberculous invasion is as a rule very slow and insidious, as if the bacillus found great difficulty in establishing itself. For weeks or months the process may continue with very little general indication. Even cough may be entirely absent, no pyrexia may be detected, and loss of weight, malaise, flushing, or occasional night sweats may be the only symptoms.

Sites of Dull Areas and Method of Percussion.

If any practitioner will acquire the habit of carefully and invariably examining the heart and lungs by accurate percussion before using his stethoscope, he will enormously increase his ability to form a correct diagnosis, and will gain invaluable indications for treatment. But at what part of the thorax may the practitioner hope to detect the earliest indications of an incipient pulmonary tuberculosis? He will naturally turn to the "apex" of the lung, and on consulting some of his authorities will be instructed to percuss above the clavicle to detect a disease which such authorities imagine to commence at the extreme summit of the lung and to advance steadily downwards. The progress of the disease has even been divided into "stages" in accordance with the distance downwards from the summit attained by the morbid process, the affection of the lower lobes being often ignored altogether in such classification. Yet it was pointed out more than twenty years ago, in a valuable paper by Sir James Fowler, that *post-mortem* examination of the precise site of the earliest tuberculous process showed that it did not begin in the summit of the lung, but at a spot about an inch and a half below the summit, from which the morbid process may extend backwards and also downwards. Sir James Fowler showed that a second localization in the outer part of the upper lobe at the same horizontal level is also extremely common, and that a third early focus is very frequently present at about an inch and a half below the summit of the lower lobe on each side.

Can we detect these local areas of incipient pulmonary tuberculosis by accurate percussion? If we are to succeed in this attempt it is essential to adopt a correct method of examination. For the investigation of the front of the chest the patient *must lie on his back on a comfortable couch*, and must be completely at his ease and with his muscles relaxed. It is impossible to obtain an accurate result from percussion of the front of the chest while the patient is standing or sitting erect; the habit of physicians to examine such patients in the erect position has been the cause of the delay in the recognition of the true picture of the localization of an early pulmonary tuberculosis. If the patient is in the recumbent position on a comfortable couch which supplies an adequate resistance, and at the same time allows of complete muscular relaxation, it is quite easy by careful percussion to detect dull areas in the first and second intercostal spaces which correspond to the statements made by Sir James Fowler, if it be remembered that a distance of an inch and a half in the collapsed lung of the *post-mortem* room will correspond to a distance of two inches or more in the air-containing lung during life. Of course, it is important to adopt a proper method of percussion. Let the practitioner practise light percussion on the terminal phalanx of a finger of his left hand firmly pressed on the spot to be percussed (the rest of that hand and forearm being kept away from the patient's chest wall). After a little careful practice of this method he will have no difficulty in determining in a case of incipient pulmonary tuberculosis dull areas in the inner and in the outer part of the first intercostal space on each side. The second and the third interspaces must then be examined in the same way, and the remaining parts of the anterior wall of the chest and also the axillary regions. For the posterior aspect of the thorax the patient should be sitting erect with his back to the practitioner; he should place his hands on the anterior aspect of the opposite shoulders, should bend gently forwards and relax his muscles. Careful percussion should then be practised over the inner and the outer parts of the suprascapular fossa on each side, and also over the posterior end of the spine of the scapula and the surrounding region. In a case of incipient pulmonary tuberculosis a dull area will be found in the inner part of the suprascapular fossa, quite close to the first and second dorsal vertebrae (a region normally resonant), which corresponds anteriorly to the dull area in the inner part of the first intercostal space. Similarly, a dull area (smaller in size) will be found in the outer part of the first intercostal space, and a very definite dull area, as large as or even larger than the area first described, will be found in the upper part of the lower lobe at the extremity of the spine of the scapula.

In examining the subclavicular dull areas more carefully, it will be found that they can usually be traced downwards into the second intercostal space, being in this space smaller but nearer together than in the first space. In a severe case the outer part of the second space may be involved, and (more rarely) the dullness may extend under the anterior fold of the axilla into the axillary region. It should also be observed that though at first the dull area in the inner part of the first space extends quite up to the sternum, as the patient improves resonance begins to appear at the sternal edge and may extend from 1 to 2 cm., so that the focus of morbid action is situated at about a fingerbreadth from the sternum.

There are, therefore, six dull areas to be detected in the upper part of the lungs in a case of incipient pulmonary tuberculosis, two in each upper lobe and one in each lower lobe. Over these dull areas the only auscultatory phenomenon in many cases is a defect in the air entry. Even the deepest possible inspiratory effort on the part of the patient produces very little inspiratory sound at these localities, while in the lower part of the lungs the air entry may be much more distinct. On careful auscultation one may sometimes detect a slight crepitant sound with inspiration or with expiration also; it may or may not vanish after the patient has coughed. Occasionally the inspiration will be slightly "wavy" in rhythm; occasionally the expiration will be slightly prolonged. At this stage of the disease there is rarely any increase in the conduction of voice sounds.

The six dull areas above described may all be present and even of considerable size, while the supraclavicular region, corresponding to the summit of the lung, may be still very fairly resonant, though the clavicle itself may yield a dull note in the positions corresponding to the dull areas below.

These six areas at the four apices are the most important and the most easily detected part of the morbid signs of an incipient pulmonary tuberculosis. They do not represent the complete picture, far from it, as I shall show presently. But they are sufficient for the diagnosis. They are, I believe, invariably present in all cases of early pulmonary tuberculosis, though in a small minority certain areas about the angle of the scapula (of which I shall speak later) may become unusually definite, especially if any pleurisy develops over them.

Diagnostic Value of Typical Dull Areas.

To prove that these typical dull areas are not produced by any other type of disease is obviously more difficult, and requires a much wider range of careful clinical observation. Yet I believe it to be the fact. The condition which may simulate them most nearly is the tendency to lobular collapse of the apices which is not uncommon in feeble children; but this condition rarely, if ever, attains to the typical symmetrical distribution of the six dull areas of pulmonary tuberculosis. An influenzal or pneumococcal bronchopneumonia does not usually affect the apices in this definite and symmetrical fashion, and the distribution of the dull areas caused by pulmonary infarcts is very different.

Extended observation is, of course, necessary to attain to absolute certainty that these six dull areas are not produced by any other infection, but at all events it is quite clear that they justify a very strong suspicion of a tuberculous infection present or past.

It must be carefully noted that the discovery of these six areas does not necessarily prove that the tuberculous infection which has produced them is active at the time when they are discovered. For, though they diminish in size as the patient improves, they do not entirely disappear. It is probable that these old dull areas caused by former tuberculous infection remain during the rest of the patient's life. They are, no doubt, in large part due to local fibrosis—small pulmonary scars. How long they may yet contain living bacilli, which may under favourable circumstances again start a morbid process, it is impossible to say. Certainly for some months after apparent recovery these areas ought to be very carefully watched, for an increase in the size of any of them may explain doubtful clinical symptoms, and may show the necessity for renewed treatment. But after a time they may certainly become completely quiescent.

It is always wise, on the first discovery of the typical areas at the four apices, to consider carefully whether there are any indications of present activity, such as tenderness, pyrexia, cough, haemoptysis, or local crepitant sounds. Whenever any reasonable ground for suspicion of active disease exists, a week or ten days of rest in bed with continuous antiseptic inhalation should be instituted as a precautionary measure. The result will often show that this treatment has been wise, for the active signs will subside, and the dull areas will become definitely smaller.

Progress of Case indicated by Measurement of Dull Areas.

When the practitioner has detected the existence of the six typical dull areas at the four apices in a case of suspected pulmonary tuberculosis, it is most important that he should carefully measure their diameter, and should keep a record of their size at the time of their first discovery, which can be compared with subsequent measurements made at intervals of three or four weeks. I have a large number of such records, and find them easy to make, and of the greatest service in prognosis and in estimating the progress towards recovery. The size can be determined with very considerable accuracy, though not, of course, with mathematical precision. To measure them in inches or centimetres would give a false idea of the amount of exactness possible under the circumstances, and would alarm the patient by the use of tape measures. But the measure by fingerbreadths is made during the routine process of physical examination and without the patient's knowledge. The fingerbreadth

is a measure which is invariable for the same observer; it is (very literally) always at hand, and it admits of quite sufficient clinical accuracy. After a little practice it is possible to estimate the size of a dull area to one-sixth or even one-eighth of a fingerbreadth, approximately equivalent to one-third and one-quarter of a centimetre.

[The lecturer then related eight recent (unpublished) cases. The first six were very early cases, in which an *x*-ray examination was made, so that the results of both methods of examination could be compared. The seventh case showed that a slight relapse, due to excess of exercise, gave evidence of increase in size of some of the dull areas, and that rest and continuance of the antiseptic inhalations rapidly brought about subsidence of this extension. The eighth was a much more chronic case, which had made remarkable improvement under the use of continuous antiseptic inhalation. The practitioner would soon find the advantage to be derived from a careful observation of these six areas and would do well at first to confine his attention to them, but there were many other positions on the chest wall where small dull areas might be detected in an early case.]

Other Dull Areas.

On the anterior wall of the chest a small dull area can be discovered by careful percussion in the third intercostal space on each side, above the nipple or somewhat more internally; and further out in the same space there is on each side a dull area below the anterior fold of the axilla. On the right side, internal to and below the nipple (in the region of the middle lobe of the right lung), there is a very definite dull area, with resonance between it and the border of the right auricle (which normally extends a fingerbreadth outwards into the fourth space, and in these cases of early pulmonary tuberculosis sometimes as much as a fingerbreadth and a half). Nothing corresponding to this area can be detected on the left side, for in this position there is the dullness due to the heart. But on both sides there is a small dull area in the anterior part of the axilla at about the level of the fourth space or fifth rib. Posteriorly there is always a dull area at the angle of the scapula on each side, and another a little externally in the posterior axillary region. When the infection in this region extends to the surface of the lung and sets up a local pleurisy, as is not uncommon, these two areas unite into one, especially if any pleural effusion occurs. If the patient improves and the effusion is absorbed, the dullness diminishes and the two separate areas can again be distinguished. But if the pleural effusion increases, it may produce a large area of dullness with defect of air entry, vocal resonance, and vocal fremitus.

If the horizontal line through these two areas be prolonged towards the spine a third area is met with on each side near, but not close to, the spine. These areas also are very constant. At a slightly higher level there is an interesting difference between the two sides, for on the left side a dull area is found at the posterior edge of the scapula, about two fingerbreaths above the angle, while on the right side the position of the corresponding area is not at the scapular edge, but always nearer to the spinal column in about the sixth space. This is a remarkable and invariable difference, which evidently depends on the different anatomical division of the lungs into three lobes on the right side and two on the left. Furthermore, in the upper part of the back on each side three small separate dull areas can often be detected, and below the angle of the scapula there are also several dull areas detectable in positions which are arranged in a kind of regular pattern.

This remarkable grouping of small dull areas may usually be detected by careful percussion in quite early cases of pulmonary tuberculosis. Their relative positions are practically invariable. From their scattered and symmetrical distribution and from the slight differences between the two lungs which I have described, it seems clear that they must correspond to the ultimate distribution of the bronchial tubes in their most direct course. The dullness and lessened air entry are probably caused by multiple local centres of infection giving rise to local collapse, followed by local bronchopneumonia of a very chronic type, and finally fibrosis.

Of these dull areas the largest are always the six at the apices, as previously described. For the apical region is the least expanded in ordinary respiration, and in a tuberculous infection of the lung (especially if it at all implicates the pleura) there appears to be a reflex inhibition of respiration, just as the diaphragm movements are checked by even a local peritonitis. In an early stage of pulmonary tuberculosis the fluorescent screen will often show a marked limitation of the inspiratory movement of the diaphragm, sometimes unequally on the two sides. There can, I think, be no doubt that a similar reflex inhibition of the other inspiratory muscles is common in pulmonary tuberculosis. Every physician must have observed how difficult it is to induce such patients to take a long deep breath and thoroughly to expand the chest. This failure of expansion in its turn makes the local conditions more favourable for the development of the tubercle bacillus—another instance of a "vicious circle" in disease. Thus the morbid process develops more rapidly in the apical areas, and these areas are the most prone to soften down into irregular cavities.

Diagnostic Value of X Rays.

[The lecturer then discussed the value of the *x* rays in diagnosis, and pointed out that this method of examination might reveal changes about the root of the lungs, opaque areas and radiating lines due in all probability to chronic morbid changes in root glands, and to peribronchial and perivascular fibrous thickening. These chronic changes might possibly in any case be very old, absolutely quiescent, and might form no part of the really active morbid process.

Very great care was required in the interpretation of the radiographic picture. It showed much which was probably of little present importance, it showed less distinctly the earliest indications of active mischief, which might be more distinctly detected by careful percussion, and it confused the localization. A practitioner who did not feel able to rely on his own percussion would do well to obtain an *x*-ray picture, taken and interpreted by an expert. But a practitioner who had learnt to trust his own percussion did not need radiography to enable him to arrive at a sound conclusion. Dr. Lees continued:]

Pathological Process.

It is clear from the percussion results already stated in this lecture, and from the *x*-ray findings which confirm them, that an incipient pulmonary tuberculosis consists in the carriage of tubercle bacilli, almost certainly by inhalation, to the finest bronchial tubes and alveoli in areas scattered freely over the lungs, followed by morbid changes which advance more rapidly in the upper part of both upper and lower lobes. From these infected areas the morbid process advances along the lymphatic sheaths of the bronchial tubes and of the blood vessels till it reaches the glands—pulmonary, bronchial, and tracheal. It seems to be possible in young children for the bacilli to make their way to these glands without any apparent implication of the pulmonary tissue. Probably this is due to the greater freedom and activity of the lymphatic circulation in early childhood.

An examination of the *post-mortem* records of the Hospital for Sick Children, Great Ormond Street, shows that in the majority of children dying from tuberculosis the chief seat of the infection is in the bronchial and tracheal glands and in the lungs. This infection is often extremely virulent, the glands being greatly enlarged, caseous, and often breaking down; frequently they involve the pulmonary tissue by contiguity or by rupture into a bronchus. (The lower lobe of the right lung is specially prone to suffer in this way.) An acute miliary tuberculosis of the lungs is a frequent result. When the process is more chronic an implication of the apices of the lungs, resembling that found in adults, may occur, and even in very young subjects cavities are not rare. In this group of cases the implication of the cervical glands is usually much less marked, while the mesenteric glands are often normal. In a smaller number of cases the implication of the bronchial and tracheal glands and of the lungs may be very slight or even absent, while the cervical glands, and still more frequently the mesenteric glands, are caseous. But even these may be but little affected while the peritoneum is crowded with tubercles, as if the bacilli made their way through the intestinal wall, leaving little or no trace of their passage, just as happens in the lungs.

The first group of cases are almost certainly due to inhalation of bacilli of the "human type," while in the second group the infection appears to enter by the alimentary canal. Probably these are often due to the "bovine" bacillus.

In adults the local chronic tuberculous changes in the lungs are much more commonly observed than in children, and the implication of the glands is much less marked and much less virulent. Also the spread of tuberculosis along the lymphatic sheaths of the blood vessels until the cerebral meninges are attacked is much less common in adults than in children. But both in children and in adults the disease begins, not at the root of the lung, but in the finest bronchial tubes and in the alveoli, in scattered areas which show a characteristic localization.

To sum up this discussion, I would claim that the first of my two questions may now be answered in the affirmative. It *is* possible for any general practitioner of medicine, by the adoption of a correct method of examination and the exercise of a little care and patience, accurately to diagnose the existence of an incipient pulmonary tuberculosis long before any bacteriological evidence can be obtained. It is not necessary for him to wait for a positive bacteriological report, and he will be foolish indeed if he allows a negative report to shake a diagnosis which is founded on an accurate physical examination. The possibility of a correct diagnosis is, in more senses than one, *in his own hands.*

II. The Treatment of Incipient Pulmonary Tuberculosis.

It is quite certain that sanatoriums have been extremely useful, and that by their help many patients have been restored to health. Yet it must also be sorrowfully confessed that many patients who have lived in a sanatorium for six months, at a very considerable cost, are still not completely cured, and that numerous instances of subsequent relapse occur, so that in the minds of many practitioners there is a growing scepticism as to the curative value of sanatoriums. And when we consider the matter carefully we see that, though the treatment by absolute rest, abundance of fresh air, and abundance of nutriment places the patient under the most favourable circumstances for fighting his battle with the invading bacilli, it does not include any organized attack on the bacilli themselves. It is the consciousness of this that has led many to desire to make trial of tuberculin. But is there no simpler and less dangerous method of direct attack? Is it quite certain that ordinary antiseptic drugs are of no value in the treatment of pulmonary tuberculosis? On the contrary, it is quite certain that some cases have markedly improved under the use of large doses of creosote. Of late the value of injections of a preparation of which iodine appears to be the active agent, has been prominently advocated in France. And a letter in the British Medical Journal from a practitioner who had been using tuberculin and whose supply was exhausted related that he had substituted injections of carbolic acid and had obtained better results. But creosote by the mouth is apt to cause much disturbance of the organs of digestion, while hypodermic injections are apt to be painful, and require the most scrupulous precautions against sepsis.

Treatment by Continuous Inhalation of Antiseptic Vapours.

Why not administer these antiseptic drugs by a method of inhalation? If a patient inhales a combination of antiseptic vapours persistently, day and night (except when actually taking food), by the principle of diffusion of gases the antiseptics must make their way into the pulmonary alveoli, if the inhaler be worn for a sufficient length of time. This is surely indisputable. And if antiseptic drugs like creosote, carbolic acid, and iodine are dissolved in alcohol, ether, and chloroform, there must be a gradual absorption of these anaesthetic agents into the blood, for this is an ACE mixture, and we know that if used in greater strength it would prove the absorption by producing general anaesthesia. The slow absorption of these agents in a very dilute condition must carry with them into the blood the antiseptics dissolved in them. How these may be transformed in the blood it is for physiological chemistry to determine, but it seems likely that some antiseptic influences will thus be carried by the blood stream to the tuberculous foci, which will then be attacked both from without the lungs and from within.

Certain other properties of this combination of drugs deserve attention. First with regard to the alcohol, we have learnt from the remarkable observations of Professor Collingwood and Dr. W. H. Willcox how much the stimulating and tonic effect of oxygen given by inhalation is increased by passing the oxygen through absolute alcohol. Dr. Willcox has calculated the amount of alcohol absorbed, and finds it to be very small; yet it exercises a most remarkable influence, as has subsequently been proved by administering oxygen bubbled through alcohol in cases of cardiac failure. The effect of the continuous inhalation of an antiseptic mixture in which alcohol is the solvent must therefore be a tonic and stimulating influence constantly at work. Another constituent of the mixture—ether—will act similarly, and perhaps more strongly. Lastly, chloroform has a local sedative influence, and strongly tends to diminish cough. I therefore recommend, and have constantly employed, the following formula for a solution to be used for continuous antiseptic inhalation:

℞ Creosoti	℥ii	
Acidi carbolici	℥ii	
Tinct. iodi	℥i	= ℥i
Sp. aetheris	℥i	
Sp. chlorof.	℥ii	

Menthol and formalin appear to me too irritating, and unsuitable for continuous inhalation.

The results of treatment which claims to be "scientific" must be tested not simply by the assertion of the patient that he is "better" (have we not all heard of the *spes phthisica?*), but by diminution of pyrexia, of cough, and of sputum, by increase in weight, and by careful observation and record of the physical signs.

[The lecturer then presented in a table (not here reproduced) the results obtained in 70 cases treated with the formula above stated, which had been already published in the medical journals.* The most recent of the cases had ceased treatment a week ago. The condition of each case in October, 1912, with very few exceptions, had been ascertained by letters directed to the patient or to the medical man who sent him. The answers had been pleasant reading, for they had shown the most satisfactory results in nearly all cases, and they had been full of gratitude. Out of the 70 cases there were:

Complete recoveries	48	
Probably complete recoveries	3	
		51
Cases incompletely cured	10	
Final result not known	2	
Died	7	
Total		70

Of the 10 incompletely cured, 9 had improved remarkably; 1 had deteriorated. Of the 7 deaths, 1 was due to apoplexy after recovery from the lung disease, 1 was hopeless when first seen, 2 abandoned treatment, 1 died from haemoptysis, 1 from abdominal tuberculosis, and 1 from tuberculous meningitis.

In 19 of the cases in this series a bacteriological examination of the sputum was made. The results were as follows:

	Cases.	Recovered.	Chronic.	Died.
No tubercle bacilli present ...	8	8	—	—
Tubercle bacilli found in 10: Small or moderate number	6	4	2	—
Many	4	3	—	1
Totals	18	15	2	1

The rapid gain in weight was worthy of notice. The gain in the first three weeks of treatment in 33 cases showed an average of 3.8 lb., the total gain in weight in 59 cases an average of 12.4 lb. In every one of the 70 cases both lungs were found to be affected, but in

* Cases 1 to 30 in the British Medical Journal, December 11th and 18th, 1909; Cases 31 to 50 in the *Lancet*, November 19th, 1910; Cases 51 to 70 in the British Medical Journal, April 6th, 1912.

almost all one lung was more affected than the other, though the difference was usually slight. In 2 cases only were the right and left lungs about equally affected. In 44 cases the right lung was more affected than the left; in 24 cases the left lung was more affected than the right. Thus in this list of 70 cases the right lung was chiefly attacked nearly twice as often as the left.

The lecturer said that the idea of administering antiseptic drugs by the air passages in cases of phthisis suggested itself to physicians thirty years ago. He referred to the use of the method by the late Sir William Roberts and the late Dr. Coghill and to the simple inhaler of perforated zinc designed by Dr. Burney Yeo in 1881.* Dr. Wilson Fox in 1885 had recommended the use of antiseptic inhalations, Dr. Beverley Robinson of New York had followed the recommendations of Dr. Yeo and had published evidence of the value of the method in phthisis and especially in laryngeal tuberculosis. Dr. Muthu had also used solutions of menthol and formaldehyde, employing them for six or eight hours daily. Dr. Garry of Cairo had also published good results, and the late Dr. Huggard of Davos had, ten or twelve years ago, used inhalations day and night continuously with success.

The lecturer then continued as follows:]

Method of Inhalation Treatment.

For the last seven years I have treated all my cases of pulmonary tuberculosis (except some seen in consultation at a very advanced stage) by the method of continuous antiseptic inhalation, by means of a Yeo's inhaler, and the antiseptic solution of which the formula is stated above. In adopting this plan of treatment it seemed to me that certain conditions were necessary if the greatest possible amount of benefit was to be obtained.

1. The inhalation *must be continuous*, the inhaler being worn all day and all night, except at meal-times.

2. At the beginning of the treatment a *period of rest in bed is essential*. The importance of this in the treatment of tuberculosis is well understood in sanatoriums. In addition the compulsory rest directs the patient's attention to the necessity for careful and thorough use of the method.

3. Digestive disturbances, anorexia, and diarrhoea or constipation must be treated by appropriate drugs.

4. The patient must be supplied with a *sufficiency of easily digested food*. Malted milk dissolved in (scalded) milk given at the end of each of the four meals has proved invaluable for this purpose.

5. *Careful disinfection of the mouth* with equal parts of Sanitas fluid and water four times daily, after the milk, has been always ordered. As soon as the patient has begun to make definite progress carious teeth should be stopped or removed. Antiseptic sprays may be used for unhealthy conditions of the fauces, and a spray of cocaine, eucalyptus, and parolein to relieve any nasal obstruction.

6. *Smoking* must be absolutely forbidden.

These are the principles on which the 70 published cases were all treated.

I advise, therefore, that whenever a practitioner has reason to suspect that a patient is infected with incipient pulmonary tuberculosis, and has by careful percussion detected the presence of the six characteristic dull areas at the four apices, he should carefully note the size of these areas, the morbid sounds present, and the patient's weight; and at once order a period of rest in bed for seven, ten, or fourteen days, according to the extent of the dull areas and the acuteness of the symptoms. Two Yeo's inhalers (each costing only 6d. or 1s.) should be procured, so that they may be used on alternate days and may be properly cleansed by immersion in boiling water and careful drying.

The solution of which the formula has been already mentioned should be supplied, and the patient instructed to pour six to eight drops on the felt of the inhaler *every hour* during the daytime and once or twice during the night if he happens to wake. Nearly all patients soon become accustomed to wearing the inhaler at night. The object should be to keep the piece of felt constantly saturated with the solution, but so that none of it wets the framework of the inhaler itself. The base of the inhaler should be carefully wiped before it is applied, and a little vaseline or lanolin cold cream smeared over the nostrils and upper lip at first.

By simple precautions like these, with the use of an inhaler of proper size, any irritation of the skin of the nose or lips may be avoided. The possibility of such irritation is the only disadvantage of the method, and it may be avoided if careful instructions are given at first. The inhaler must be worn persistently and removed only at meal times. After the first day or two the patient, while in bed, may read, write, do hand work, or play simple games. Dyspepsia and anorexia may be treated with sodium bicarbonate, nux vomica, and a vegetable bitter given a quarter of an hour before meals three times daily. The patient should be encouraged to eat freely of plain nutritious food, and at the end of each of his four meals he should drink slowly a half-pint of fresh milk which has been raised to the boiling point and allowed to cool, and to which two teaspoonfuls (gradually increased to a tablespoonful) of malted milk has afterwards been added. The bedroom windows should, of course, be kept open, but the patient must be kept warm in bed and protected from draught. After his meals he should always rinse his mouth with equal parts of Sanitas fluid and water. Smoking must be absolutely forbidden. When the necessary period of rest in bed is at an end the patient may be allowed to sit up in his room still wearing the inhaler persistently. Two or three days later he may be allowed to lay it aside for an hour in the morning and take a short walk out of doors. But when he returns he must at once replace his inhaler. At the end of three weeks the practitioner should again weigh his patient and again carefully record the size of his dull areas. Almost invariably a considerable gain in weight, often from 3 lb. to 5 lb., will be discovered, and the size of the dull areas will be found to be definitely smaller. Cough will be much diminished—in many early cases it ceases altogether; sputum will be less and more easily expectorated; and the temperature chart will be more satisfactory. The patient generally feels so much better that he is quite willing to continue the treatment. He should now be instructed to practise simple breathing exercises three times daily in order to obtain a better expansion of the lungs. The inhalation treatment should be continued persistently (except for an hour in the morning) during the next four weeks, at the end of which time the weight should again be taken, and the size of the dull areas carefully ascertained and recorded. If the progress of the case has been satisfactory during the first seven weeks it may now be possible to reduce slightly the number of hours of inhalation, but such reduction should be very gradual. The danger at this period is that the patient, feeling himself better, should grow careless. If he does, there is a considerable chance of a relapse, which will necessitate a return to bed for a fortnight with continuous inhalation. Experience shows that the patients who are most conscientious and persevering in carrying out the treatment improve most rapidly and are least liable to relapse. A very early case may, perhaps, be able to return to ordinary life after three or four months of treatment, but one in which cavitation has occurred will need six months or longer. After apparent recovery the patient will do wisely to use his inhaler for one or two hours daily, and also at night, for a further period of six months. Whenever he is conscious of having "caught cold," he should remain in bed for a day or two days, and practise continuous inhalation all the time.

This treatment by antiseptic inhalation can be carried out in the patient's own home if the hygienic and sanitary conditions are satisfactory. It is not usually necessary to send the patient to a sanatorium, so that there is a great saving in expense, and the practitioner does not lose charge of him. But where the home conditions are unsatisfactory it will be wise to send him to a sanatorium at which he will be permitted to carry out the treatment by inhalation. No idea of change of climate or foreign travel or sea voyage should be entertained until he has been through a course of inhalation treatment, and even then he must take his inhaler with him and use it freely.

I now venture to claim that my second question, as well as the first, may be answered in the affirmative. It *is* possible to provide the practitioner with a simple, inexpensive, and very effective form of treatment which can be used in the patient's own home.

* It had been found advisable to increase the size a little for cases with prominent noses. This model is known as Yeo's inhaler (M), the original type as Yeo's inhaler (F); both are made by Squire and Son.

EIGHTIETH ANNUAL MEETING

OF THE

British Medical Association.

Held in Liverpool on July 19th, 20th, 22nd, 23rd, 24th, 25th, and 26th.

PROCEEDINGS OF SECTIONS.

SECTION OF PATHOLOGY.

Professor Walker Hall, President.

PRESIDENT'S INTRODUCTORY REMARKS.

In the course of a short address initiating the proceedings of the Section, and after allusion to the fact that a number of the most eminent authorities on the subjects to be discussed were present, Professor Walker Hall said: We are sometimes apt to forget that the Annual Meeting of the Association affords us the important advantage of coming into close contact with those who are working on the advancing line of knowledge in the subjects selected for discussion. Although through the medium of our Journal a summary of the proceedings may be read in the remotest hamlet or colony, the mere printed account does not convey the stimulus obtained by the actual participation in the discussions. Such a stimulus tends to stir us from the grooves and ruts of everyday routine, and enables us to look at ordinary things through other glasses. If I read aright the signs of the times, a demand for post-graduate study and stimuli is being more widely prepared, and the universities will find it necessary to provide courses to suit the needs of their particular districts. It may happen that social developments will lead to the distribution of official and semi-official appointments upon the merits of post-graduate attainments, and so make our life-work one of repeated periods of study as well as of daily bedside observation; but whether this be an incentive or not, there is every reason to suppose that it will be more and more necessary to keep abreast with the advances of laboratory investigations; hence these meetings will gain importance in the eyes of the members of the Association engaged in general practice. On the other hand, the laboratory worker finds it of great value to meet and to talk with those who are confronted with the problems of the individualities as well as of the diseases of patients, and we may reasonably hope for larger interest from this quarter as well. Our programme has been arranged with the deliberate idea of bringing forward subjects of everyday interest to the general practitioner.

DISCUSSION ON BRIGHT'S DISEASE.

OPENING PAPERS.

I.—J. Lorrain Smith, F.R.S.,

Professor of Pathology, University of Manchester.

It is admitted on all hands that the correlation of the clinical and pathological findings in cases of Bright's disease is one of the most difficult questions in medicine, and this difficulty arises from the complexity of the problem.

In the kidney there is a very complicated secreting apparatus, and a blood supply in which the blood passes through two sets of capillaries, as well as a lymph circulation the conditions of which are very little understood. To explain how each of these elements is affected in the condition of inflammation is the problem presented to the pathologist.

For some time I have been making observations on granular kidneys, and it is with this subject that I propose now to deal.

The result on the kidney of the various inflammatory lesions that affect it is the atrophy, degeneration, and disappearance of the secreting tubules, the thickening and obliteration of the lumen of the blood vessels, and, on the other hand, there follows the growth of the fibrous tissue stroma; the fibrous tissue survives and grows while the other tissues atrophy and disappear. The presence of fibrous tissue, beyond the very limited amount which is normally present, indicates that, in the area where it is found, there has been, in some form or other, a preceding inflammation, and it is worth while considering the problem of Bright's disease from the point of view afforded by a study of the distribution of fibrous tissue.

It is usual to discuss nephritis as it takes the form of an acute attack with more or less direct sequelae, and the problem then is to discover on which particular structures in the kidney the attack has fallen. It may fall on the glomeruli, on the tubules, or the stroma, and it is from a study of the problem in this way that we have learnt most of the known facts; but there are limitations to the application of this conception due to the fact that the various structures can be only partially separated from each other, and therefore, when the function of one structure is disturbed, the others suffer also, though it may be in a less degree. Inflammation tends to cause necrosis especially of the epithelium, and following on this there is a growth of the fibrous stroma, and by the growth the stroma replaces the tissues which have been destroyed. It is, however, not an equivalent replacement, and the organ as a whole decreases in size. In this respect nephritis resembles infarction of the kidney, where the fibrous growth which follows is always small in amount. So also the fibrous overgrowth following arterio-sclerosis of the larger renal vessels is always scanty, and there are large depressions in the cortex corresponding to the arterio-sclerotic areas. In the kidney there is observed also an abundant growth of fibrous tissue in cases of chronic parenchymatous nephritis, where the tubules show atrophy of the epithelium and degeneration. The exact stimulus to the growth of fibrous stroma in such a case has never been determined. Necrosis, if it occurs in such cases, can be only of the partial kind described by Albrecht, in cloudy swelling.

If we leave aside the controversies as to the exact sequence of events in various types of nephritis and compare the different forms as regards the distribution of fibrous tissue during the chronic stages of the disease, we find that there are two types of fibrosis: (1) That in which the fibrous tissue appears diffused through the cortex more or less uniformly; (2) that in which the fibrous tissue is distributed in patches, while the other areas of the cortex are unaffected. The stroma has not increased at all in certain areas, while in the affected areas it has increased very markedly, and replaces, more or less completely, the tubules and glomeruli and blood vessels. It is, therefore, a circumscribed or patchy fibrosis.

Diffuse Fibrosis.—This form is seen in its earliest stages in the large white kidney. If the nephritis is of comparatively recent origin, say of six months' standing, the kidney is large and pale in colour; the capsule is not thickened nor adherent; the cortical surface is quite smooth and the organ has the appearance of degeneration rather than of fibrosis. The microscope shows the presence of a large amount of fibrous tissue distributed uniformly throughout the cortex and in such a way that in a section it would be impossible to find a tubule or glomerulus or blood vessel that is not surrounded by a layer of fibrous tissue. From this change in the structure we may infer that the function of the tubules is interfered with, because it is obvious that the tubule will lose in efficiency when separated from its blood vessels by a strand of fibrous tissue. We may also infer that the nephritis affected the whole structure of the organ, and has left as a sequela a correspondingly general fibrosis.

The tubules show characteristic changes: the epithelium is very much atrophied, the part of the cell protoplasm next the lumen having broken away, and the lumen has the appearance of dilatation. The glomeruli are, as a rule, enlarged, and some of them may show signs of the fibro-hyaline transformation associated with nephritis of long standing. There is also marked fatty degeneration. Globules of fat are seen in the secreting epithelium very generally, and they are also plentiful in the stroma. The

appearance suggests that the globules in the stroma are derived from the epithelial cells of the tubules from which they have passed to the spaces of the stroma.

It is the abundance of fat in the epithelium and interstices that has given the name of "large white kidney" to this condition. At the stage which I have described the fibrous tissue has not contracted. It is still oedematous and soft, so that the cortical surface has as yet no granularity.

The functional changes correspond with the alterations in structure. The urine is scanty and loaded with albumen and casts; the patient has general oedema, and finally dies of uraemia. The secreting power of the kidney, both as to solids and fluids, has been damaged, and as we have seen from a study of the fibrosis the whole kidney is diseased. The kidney contains no tissue which can carry on the function, still less is there any secreting tissue which shows signs of hypertrophy. There is everywhere atrophy of the epithelium and everywhere fibrosis. The patient with this type of disease as a rule survives only a short period. Many die before the subacute stage is reached, a considerable proportion die in the subacute stage a few months after the acute attack; rarely does the patient live for many years.

It is to be noted, however, that patients do occasionally survive, and we must also point out that while the whole

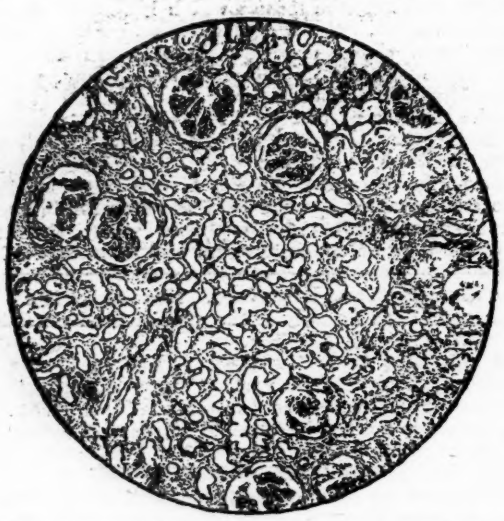

Fig. 1.—Section of kidney cortex, low power, in a case of large white kidney. The patient was a boy aged 13, who died nineteen weeks after the acute attack. Atrophy of epithelium and growth of intertubular stroma and fatty degeneration in epithelium and stroma universal throughout the cortex.

kidney is diseased it is evident, as we pass from area to area in examining the cortex, that one area is more severely damaged than another. The fibrous new growth is more plentiful in one area than another. It is probably not going too far to suggest that a kidney with a large amount of relatively less affected tissue is able to maintain the renal functions for a correspondingly longer period, and therefore the period of survival varies.

Patchy Fibrosis.—The kidneys are small in size, weighing 2 oz. and 3 oz. each instead of the usual 5 oz., and it is to be remembered that even this reduced kidney contains much functionless fibrous tissue in its cortex. The cortical surface is granular and the sulci between the granules are red, while the granules are pale. The redness which has given the name of "small red kidney" is due to congestion of small capillary-like vessels in the fibrous strands, which are widely dilated with blood and are thereby a great contrast to other vessels in the same area, with very much thickened walls and narrowed lumen. The cortex generally is diminished in size, and the mouths of the blood vessels are prominent and rigid. The cortex may be pale in colour, partly from the absence of the congestion described and partly from the presence of fatty degeneration. This, however, is never so marked as that seen in the cases of diffuse fibrosis.

The microscopic examination shows as an outstanding change in the structure the presence of areas in which fibrous tissue is abundant and where it has led to atrophy, compression, and disappearance of the tubules. The fibrous tissue has not only surrounded them, but it has contracted and led to their compression. The glomeruli in this area are transformed into masses of hyaline fibrous tissue. Bordering on the areas just described, and roughly corresponding to them in size, are areas of cortex in which there is no fibrosis whatever. The tubules are in the normal relation to each other, and to the blood vessels, and those and the glomeruli are normal in structure. The tubules in which the normal relationship has been preserved may show cells in a condition of hypertrophy. The disease in this case is a localized circumscribed effect, and the hypertrophy of the epithelium is due to an effort on the part of the surviving normal tubules to compensate for the loss of those which occupied the areas now in a state of fibrosis. In such a case the kidney secretes a large amount of urine, but it contains little or no albumen, and casts are rare. The blood pressure gradually rises, and the patient dies from uraemia, cerebral apoplexy, acute infections, such as pneumonia, pericarditis, pleurisy, etc. As a rule, the disease runs a prolonged course, and the fact that the patient survives is due to the presence in the kidney of islets of normal secreting tubules and glomeruli, which are so normal that

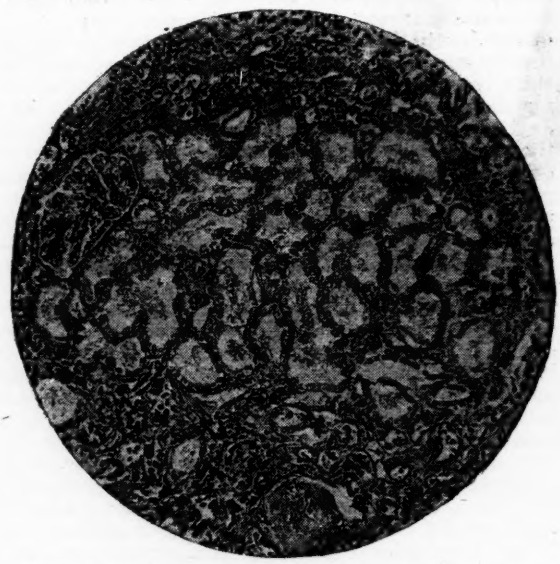

Fig. 2.—Low power magnification of section of cortex in a case of chronic interstitial nephritis associated with gout. In the centre is an islet of normal cortex, showing a normal glomerulus with more or less normal tubules (dilated). Surrounding this are fibrous areas in which the stroma has increased, the tubules have atrophied, and the glomeruli are hyaline and fibrous.

they can pass into a condition of hypertrophy. Since it is on these islands that the prolongation of the patient's life depends, I need not point out that the efforts of the practitioner in attendance are and should be directed to the maintenance of them intact. The patient, however, has a diminished amount of kidney tissue, and a strain which could produce no harmful effect on a normal person, such as muscular exercise, will give rise to albuminuria in a patient with a kidney of this kind. The question naturally arises, Is this merely albuminuria, or have we to deal with the kind of effect from which springs the gradual slow progress of the nephritis in such cases? This is a question of fundamental importance in practice; but data are not yet available to furnish the answer.

So far I have dealt with the problem as it is presented in the contrast between large white kidney and granular kidney. The large white kidney, however, is but a stage in the development of the sequelae of parenchymatous nephritis.

If we take the other stages of the condition we find the same type of structural changes present. In acute scarlatinal nephritis, for example, we may find very marked exudation, not only of fluid but also of cells, into the renal tissue. This is diffuse, but at the same time one area as compared with another may be so intense as to have a focal appearance.

The difference is brought out in the photograph from a case of scarlatinal nephritis. In the centre of intenso

reaction the tubules have disappeared and have been replaced by the cells of inflammatory exudation, but in neighbouring areas the tubules still remain.

The same contrast is brought out in the small white kidney which is the more chronic form of the diffuse nephritis. Here the changes, though diffuse in distribution, vary in degree from one locality to another. This is brought out in the accompanying photograph (Fig. 4).

It has been held by various pathologists that the chronic interstitial nephritis or, as I prefer to call it, localized nephritis, has its origin in vascular disease. It is so different from the parenchymatous type in its form and development that its origin in the same kind of nephritis seems at first sight improbable. Does it ever happen that a case of undoubted diffuse nephritis terminates in a genuine granular kidney of chronic interstitial nephritis? This is difficult to prove; so difficult, indeed, that Sir Clifford Allbutt is inclined to doubt his former belief in the sequence.

That diffuse nephritis recovers completely in certain cases, however, is admitted, and we can conceive of cases in which recovery was irregularly complete; where certain areas completely recovered, while others passed into a state of fibrosis, with complete destruction of function following. That such is the case would seem to be the explanation of the late Dr. Dixon Mann's well-known case of post-scarlatinal nephritis. He published an account of

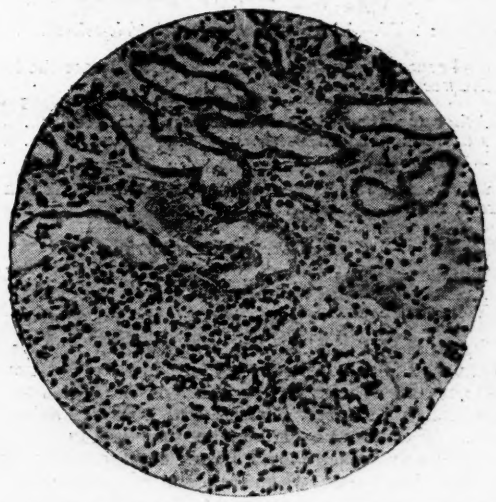

Fig. 3.—Section of kidney cortex, low power, from a case of scarlatinal nephritis. The cellular reaction is intense and universal, but varies from one area to another.

this case, along with a report on the histological changes by Professor Delépine, in the *Lancet* of 1895:

> The patient had scarlet fever, and developed nephritis of the typical parenchymatous form. There was only partial recovery, and albumen was found in the urine at the six to eight examinations which Dr. Dixon Mann carried out each year for a period of 28 years. The patient died of uraemia. The kidneys weighed about 2 oz. each, and they showed adherent thick capsule, contracted cortex, and granular cortical surface.

This case shows on microscopic examination a form of patchy fibrosis, but, as might be expected after so long a period, the fibrosis has invaded a very large part of the cortex.

The case affords as clear proof as we could obtain that a parenchymatous nephritis may subside to such an extent that patches of cortex are left in a condition approaching the normal. This is not a commonly observed sequence, and it would be worth while for clinical observers to record cases which clearly illustrate this sequence.

Such a sequence cannot be shown in the ordinary cases of chronic interstitial nephritis; and, indeed, the time and mode of onset in this disease are, in the majority of cases, exceedingly obscure. We cannot, however, attach much weight to this argument, since Sir John Rose Bradford has shown that there is a type of diffuse parenchymatous nephritis in which there is not only an absence of symptoms till near the end of life, but the patient can give no account of the onset. The origin of the conditions which lead to granular kidneys is not more obscure than this.

The explanation of granular kidney as the result of arterio-sclerosis of the small arteries and capillaries has a certain amount of evidence in its favour. It is in accord with the insidious origin of the condition; the localized fibrosis is, in many respects, similar to that seen in cases of multiple small embolisms; the absence of symptoms in connexion with the function of urinary secretion and the great prominence of the vascular changes also find a very simple explanation.

On the other hand, these arguments are not altogether convincing. The localization of the disease may be due to other causes than the blood vessel distribution. In pyelonephritis, such as that accompanying stone, or in tuberculous pyelonephritis, there is a form of localized patchy inflammation and fibrosis where the distribution of the inflamed areas is not determined by the blood vessel distribution.

As regards changes in the urine, one need only point out that they are never quite completely absent. There may for a period be no albumen or casts in the urine, but it is not unusual for both to be found, and any strain on the kidney may be followed by the appearance of albumen. On the other hand, in the quiescent periods we would not expect to find albumen and casts in the urine since the

Fig. 4.—Section of the kidney cortex in a case of small white kidney. There is universal fibrosis, but there is considerable variety in the extent to which different areas are affected. In contrast to the large white kidney section this shows more compact fibrous tissue and advanced hyaline fibrous changes in the glomeruli. There is a clear history of well-established symptoms six months before death.

urine is secreted by glomeruli in tubules approximately normal, and those areas or patches affected are completely functionless. In other words, the damaged tissue which might secrete abnormal urine is in such a condition of atrophy that it does not secrete at all. Finally, the prominence of the blood vessel changes in granular kidney is admitted, but the explanation that suggests itself for this is that in these cases the process has been at work for a long period of years, and the slowly formed sclerosing process and the accompanying degeneration have had ample time to form. Vascular and glomerular changes, similar though not so marked, are to be found in the small white kidney.

To enable us to come to a clear decision on this question it would be necessary to have further insight into the meaning of localization of structural changes in general as the result of a general pathological process. A similar difficulty arises in connexion with widely different forms of disease. Dr. Rolleston has pointed out that a comparison may be drawn between the conditions of the fibrosis (diffuse) in hypertrophic cirrhosis of the liver and in the atrophic (localized) form. In the latter case there is a patchy lesion in the areas of which the secreting liver tissue is being destroyed, and, alternating with this, islands of approximately normal and active liver cells, which not infrequently show some degree of hypertrophy. In the case of hypertrophic cirrhosis, again, the growth of

fibrous tissue is more rapid and more diffused, and this tissue being the more recently formed, is vascular and cellular rather than fibrous. The localization of the process in these contracted conditions is exceedingly obscure.

Another comparison from a different field of pathology is obtained in studying fatty degeneration of the heart. In one type the fat is deposited in the muscle fibres in well defined areas and in the other it is present in a diffused form and is found in each fibre. Here also the localization remains to be explained.

It is unnecessary to adduce further illustrations of the localization in organs and tissues of effects which are due to generally distributed toxins or other irritants, and it is impossible here to enter on a fuller discussion of the physiological basis of this form of localization. My purpose has been to show that in the contrast between the localized and diffused fibrosis we find one of the most definite and significant distinctions in the pathology of nephritis, and a distinction which must be kept in view when an attempt is made to classify in detail the various forms of Bright's disease.

In conclusion, I would like to put before the Association an analysis of the records of 1,710 *post-mortem* examinations carried out in recent years in the Royal Infirmary, Manchester, and I have to express my indebtedness to Dr. Anderton for his help in working out the results.

Of these cases, 395, or 23 per cent., showed the presence of granular kidney at the time of death. Of these, 276 were uncomplicated cases of Bright's disease. So that about 16 per cent. of all the cases died from Bright's disease.

TABLE I.

In 1,710 consecutive *post-mortem* examinations the kidneys were found to be granular in 395 cases, or 23 per cent. This group includes cases of chronic interstitial nephritis, small white kidney, and arterio-sclerosis of the kidney. Of these cases, 62 per cent. were men and 38 per cent. women.

The age distribution of the cases was as follows:

Age.	Per Cent.	Age.	Per Cent.
10–20	0.25	50–60	26.25
20–30	2.25	60–70	23.5
30–40	10.50	70–80	6.25
40–50	28.75	80–90	0.75

The period during which granular kidney is most common is from 40 to 70 years of age. After the age of 70 there is a very marked decrease in the number of cases observed. The decade of 40 to 50 is the period during which the largest number of cases occur.

TABLE II.—*The Immediate Cause of Death.*

	Per Cent.
Uraemia	6.75
Cerebral haemorrhage	14.0
Heart failure and diseases of vessels	33.0
Infections, etc., of lower genito-urinary organs	1.75
Acute general infections	11.75
Tuberculosis	2.75
Other causes, including accidents	30.50

The cases of heart failure, etc., may be subdivided into:

	Per Cent.
1. Chronic cases associated with general passive congestion	20
2. Cases of acute failure from myocarditis, malignant endocarditis, atheroma of the coronary arteries, aneurysm of the aorta, etc.	13

In this table we have placed cases of cerebral haemorrhage in a separate group; they should, however, be considered also as belonging to the group in which the cause of death is to be found in the disease of the circulatory system. When the two groups are added together they amount to 47 per cent. of the total, or nearly half the number.

Table III shows an interesting uniformity in the average weight of the hypertrophied heart at the different periods. It would suggest that the capacity of the heart to hypertrophy is limited in some definite way, either by the limitation of the demand or by a limitation of the response on the part of the muscle because of the abnormal metabolic conditions of the organism. It is clear, however, that the response is not so much limited in conditions of lead poisoning.

TABLE III.—*Average Weight in Ounces of the Heart in all Cases.*
Normal = 11.

Age.	Oz.	Age.	Oz.
10–20	11½	50–60	15.4
20–30	14½	60–70	14.5
30–40	16.1	70–80	14.5
40–50	14.3	80–90	14.3

Of the whole 395 cases, 276 were uncomplicated by any other disease, and if these be taken separately the weight of the heart averages 14.85 oz.

The average combined weight of kidney in these cases, if we exclude those of active or passive congestion, was below the normal average of 9 oz. in 75 per cent.

Among the 1,710 cases, 41 were from tradesmen in whose occupation there was a risk of lead poisoning.

Number of cases	41
Granular kidney	21
Average weight of heart	18.8 oz.

The average combined weight of the kidneys in these cases was 8.7 oz.

TABLE IV.—*The Association of Arterio-sclerosis with Granular Kidney.*

In 144 cases uncomplicated by any other disease the following results were obtained:

	Per Cent.
Atheroma of aorta, cardiac and cerebral arteries	30.6
Atheroma of aorta alone	15.0
No atheroma	22.0

In the remaining 32 per cent. atheroma occurred in one or more of the three sets of vessels.

Cases of Cerebral Haemorrhage.

Number of cases	55
Average combined weight of kidneys	7.8 oz.
Weight of heart	15.2 oz.

The analysis of the results as regards disease of the blood vessels is of interest. There is no atheroma visible to the naked eye in 22 per cent. of the cases where an examination of aorta, cardiac and cerebral arteries was made. This is remarkable, because it has been shown that granular kidney is not a very common occurrence till after 40 years of age, when the time of arterio-sclerosis is approaching. That so large a percentage should show so little evidence of arterio-sclerosis is confirmed by the fact that in a great proportion of the remaining cases arterio-sclerosis was of a very limited type, occurring in only one of the three sets of vessels.

Finally, the cases of cerebral haemorrhage are characterized rather by the small size of the kidneys than by the excessive hypertrophy of the heart muscle, but on neither point do the results show striking differences as compared with the other cases.

II.—Sir Clifford Allbutt, K.C.B.

Sir Clifford Allbutt, in a paper read for him by one of the honorary secretaries, dwelt on the cardio-vascular changes, first attempting, however, to simplify the nomenclature, which was unnecessarily complicated, different names often being used to denote the same lesions. He discussed the condition of "granular kidney," and disbelieved entirely in the condition described as "potential Bright's disease" by Straus. It was not a renal condition in any sense. Hyperpiesis he considered to be a malady, not "granular kidney," but resembling it in the increased blood pressure and the proneness to apoplexy, but was curable.

III.—J. F. Gaskell, M.D., M.R.C.P.Lond.,

Beit Memorial Research Fellow.

The thesis[1] that I desire to maintain is that in the conditions grouped under the term "Bright's disease"

[1] The evidence on which rest the conclusions reached in this paper will be found in the *Journal of Pathology and Bacteriology* for January, 1912.

permanent damage to the kidney is always a result of glomerular changes. These glomerular changes may arise in two ways: (1) As the result of inflammatory processes; (2) as the result of vascular alterations. On these lines a satisfactory grouping of the morbid anatomical conditions may be arrived at which is in accordance with clinical experience.

Temporary changes in the tubules which give rise to a transient albuminuria are extremely common, and arise from a great diversity of causes, but do not, in all probability, give rise to permanent changes and are completely recovered from. They arise during the course of any febrile disease, and also as the result of the introduction and subsequent excretion of poisons gaining access to the circulation. In this group come the conditions set up experimentally by various poisons, conditions which, therefore, have no bearing on nephritis proper. The most extreme example of this type of change is met with in the toxaemia of pregnancy; the poisoning of the tubular epithelium here may be so acute that the whole cortex of the kidney becomes necrotic; yet if the glomeruli be examined they will be found to be unaltered. The change in this group is due to the excretion of toxic material through the normal excretory channels—namely, the epithelial cells of the tubules; the group may therefore be called the "toxic group." As has already been stated, these changes, if not accompanying a fatal termination, are in all probability completely recovered from, the tubular lining cells possessing a remarkable power of completely reclothing the tubules of the organ. Interstitial changes are slight or absent in this group.

I will now consider the group of cases which are of a true inflammatory type; it includes three forms:

(*a*) *Glomerulo-tubular nephritis*, a general inflammation of the whole organ;
(*b*) *Embolic focal nephritis*, a form peculiar to cases of ulcerative endocarditis;
(*c*) *Acute interstitial nephritis*.

Glomerulo-tubular Nephritis.—This form, in my opinion, represents nephritis proper and is essentially an inflammation of the whole organ, all structures being affected, both all tubules and all glomeruli. As in inflammations elsewhere, it is accompanied by a marked inflammatory exudate which gives rise to an oedema of the whole organ, and is mainly responsible for the marked increase of size found in the cases of shorter duration. The glomerular changes are characteristic. Three distinct stages of this condition are met with:

1. The early acute stage, found in patients dying within a week or two of the onset of the disease. The organ is tensely swollen and oedematous, and red points may be seen over the surface consisting of haemorrhages into the glomerular capsule space and the convoluted tubules belonging to it, which surround it. The glomeruli are all swollen and tensely fit the capsule space or even extend down the first part of the convoluted tubule. There is considerable interstitial oedema and the tubular epithelium is swollen and desquamating. This stage is clinically haemorrhagic in type.

2. The subacute or reparative stage, found in patients dying some months after the onset of the disease. The condition is commonly known as chronic parenchymatous nephritis or "mixed" nephritis. The glomeruli are *all* altered and often show points of adherence of one or more capillaries to Bowman's capsule. The interstitial oedema is very great—it is, in fact, comparable to great general oedema found throughout the body in these patients. There is also a considerable formation of new fibrous tissue. This interstitial increase is, to a great extent, the cause of the great increase in size of the organ. The tubular epithelium is in all stages of degeneration and desquamation, though at the same time a fairly complete layer of flat young epithelial cells may be observed lying on the basement membrane. The distension of the tubules with fluid and débris partly accounts for the increased size of the organ. This form is clinically albuminuric in type.

3. The chronic or sclerotic stage, occurring in patients whose primary attack took place a year or many years before; often known as secondary contracted kidney or small white kidney. The organ is greatly contracted, owing to the contraction of the interstitial fibrous tissue and the disappearance of the interstitial oedema. A large number of glomerulo-tubular systems are destroyed; these glomeruli are hyaline in appearance and are blended with the surrounding tissue, which consists of fibrous tissue containing atrophic remains of the tubules. The still existing glomeruli have recovered as far as is possible, but usually show signs of previous inflammatory changes in points of adherence to the capsule. The capsule space is thus divided into pockets, which now have become lined with new capsule epithelium. Any fibrous strands running across the capsule space become also similarly lined; this may give rise to the appearance of laminated cell layers occupying the capsule space. The tubules belonging to these glomeruli are greatly distended and lined by a flattened epithelium. These patients usually die from uraemia, a termination which can be explained on the lines of Rose Bradford's experiments as being due to the destruction of a sufficiently large amount of kidney substance.

This sclerotic stage of glomerulo-tubular nephritis is always accompanied by cardio-vascular alterations. The blood-pressure is increased and the heart is hypertrophied, the hypertrophy being specially marked in the wall of the left ventricle. Experiments by Pässler and Henle have bearing upon these changes, as they tend to show that heart hypertrophy can be experimentally produced by the removal of just a little less kidney substance than is necessary to produce the uraemia of Rose Bradford's experiments.

The other two inflammatory forms may now be briefly considered.

"*Embolic focal*" nephritis occurs always in connexion with ulcerative endocarditis, and is due to the lodging of minute emboli from the valves in one or more capillaries of the glomerular tuft. The glomerular picture is therefore characteristic; one part of the glomerulus is the site of what is really a small abscess, while the rest of the capillaries are normal. Another characteristic is the occurrence of a varying number of normal glomeruli—that, is to say, the changes are found only in a portion of the glomeruli and only in a portion of the glomerular tuft, instead of being diffuse throughout the organ as in glomerulo-tubular nephritis. This form is essentially haemorrhagic in type. As recovery practically never takes place in ulcerative endocarditis, it does not have the opportunity of giving rise to a chronic lesion.

"*Acute interstitial*" nephritis occurs in the early stages of scarlet fever, giving rise to few or no symptoms. It is characterized by infiltration of the interstitial tissue round the vessels and round the glomeruli with cells, mainly of the plasma-cell type, the glomeruli themselves being unchanged. This change is not peculiar to the kidney, but occurs also in the liver and other organs. As the patients in whom this form has been found *post mortem* have always died in the early stages of the disease, it is improbable that any secondary chronic conditions arise from it.

The vascular group may now be discussed. This subdivides into two forms: (1) The senile arterio-sclerotic type; (2) the "primary" contracted kidney.

Senile "Arterio-sclerotic" Kidney.—This is by far the commonest form of contracted kidney found; it is characterized by its extremely small clinical significance. The changes are due to the spreading in of those senile degenerative arterial changes which start in the aorta and occur to a greater or less extent in all old people. The changes spread into the interlobular and afferent arteries of the kidney in an extremely irregular manner. The destroyed atrophic glomeruli are arranged round and belong to those interlobular arteries which are most markedly affected. The nodules formed by the atrophy of the glomerulo-tubular systems are therefore arranged according to the distribution of the interlobular arteries—that is to say, in a wedge-shaped manner. Now this form of kidney contraction is not accompanied by any great heart hypertrophy or any increase of blood pressure. The pressure in the glomeruli belonging to the more diseased interlobular and afferent arteries therefore falls below the 30 mm. necessary for secretion, the function of the glomerulus and its corresponding tubule disappears, and the system shrinks and atrophies. The requisite excretion of urine is easily accomplished by the many sound glomeruli belonging to the better interlobular arteries. The clinical symptoms in this form are very slight,

polyuria and a slight trace of albumen, especially in the early morning urine, being the main ones. The condition has, as has been already mentioned, very little clinical significance apart from the general arterio-sclerosis.

Primary Contracted Kidney.—This is known in Germany as genuine "*Schrumpfniere.*" Though of vascular origin, it differs widely in its clinical significance from the previous form; it is at the same time far more uncommon in a form so advanced that the kidney is microscopically altered, as other organs are also affected, notably the brain, and death frequently occurs before the kidney change is sufficiently advanced to be observable to the naked eye. The condition is essentially primarily a vascular one, and consists in a peculiar change occurring in the small arterioles of the kidney, brain, spleen, and possibly other organs. The change takes the form of a great increase of the intimal layers of the arterioles accompanied by a considerable deposit of fat in these layers. It is according to Jores, who first correctly described it, a true arterio-sclerotic change. Its incidence is, however, on an entirely different part of the circulation to that in the senile form; the former attacks first the smallest arterioles, the latter the largest arteries, in particular the aorta. Accompanying this arteriole change occur great increase of blood pressure and great hypertrophy of the heart. This is the condition in which those enormous hearts are found which have been referred to by some authors as an idiopathic hypertrophy of the organ. The changes in the small arteries of the brain usually lead to the common cause of death—namely, haemorrhage into the basal ganglia of the brain, the pons, or the medulla. The kidney changes are in many cases macroscopically unrecognizable, as they consist merely of increased thickness of the wall of the small arterioles with fat changes. It is only when the arteries are so thickened that the lumen is obliterated that the glomeruli lose their function and are destroyed; for the high blood pressure is able to supply blood to the glomeruli at sufficient pressure for secretory purposes through an artery with a very great diminution of its lumen. Inasmuch as the change probably comes fairly suddenly, the secondary inflammatory reaction is usually well marked, and thus gives rise to appearances suggesting an inflammatory cause for the condition.

To recapitulate, the condition of the kidney is characterized microscopically by great thickening of the arterioles, a change which is frequently found in kidneys macroscopically normal. This arteriole change is accompanied by high blood pressure and great heart hypertrophy, which frequently lead to death from cerebral haemorrhage. The age of these patients is younger than that in the senile form, usually ranging from 40 to 55; the senile changes are rarely well marked in patients under 60.

In conclusion I will state in tabular form my classification of those forms of kidney disease which I have considered:

1. Toxic—due to exogenous or endogenous poisons.
2. Inflammatory.
   - (*a*) Glomerulo-tubular nephritis. Nephritis proper.
     - (1) Acute.
     - (2) Reparative. Large white or mixed nephritis.
     - (3) Sclerotic. Secondary or small white.
   - (*b*) Embolic focal nephritis. In ulcerative endocarditis.
   - (*c*) Acute interstitial nephritis. Scarlet fever, early stage only.
3. Vascular.
   - (*a*) Arterio-sclerotic contracted kidney. Senile form.
   - (*b*) Primary contracted kidney. High-pressure form.

Throughout this paper I have purposely avoided using the terms "chronic interstitial nephritis" and "red granular kidney," as they are indefinite terms used in many different senses by different observers.*

DISCUSSION.

Dr. THEODORE SHENNAN (Edinburgh) said: I have thought it might be a useful contribution to the discussion if I placed before you a few of the difficulties which are met with in the *post-mortem* room in the diagnosis of morbid changes in the kidney, and which, in place of lessening with experience, either remain, or tend to present themselves under new aspects. One must, of course, assume that we all admit that mixed forms of nephritis are the rule, and when a separate form of nephritis is named, this means only that a given change predominates. An important difficulty is the nomenclature. Terms such as "large white kidney," "small white kidney," "small red kidney," "granular contracted and contracting kidneys," "subacute diffuse nephritis," "chronic parenchymatous nephritis," are not used by all pathologists to indicate the same morbid changes, and, still worse, the pathologist's connotation of some of these terms does not always correspond with that of the clinician. The first five of these terms should be abandoned, or at least modified; or, preferably, descriptive anatomical terms substituted. I object especially to the term "large white kidney," which is applied not only to kidneys the seat of subacute parenchymatous nephritis, but also to amyloid (waxy) kidneys in which there is a large amount of chronic parenchymatous or interstitial change. "Small red kidney" and "granular contracted kidney" may mean either arterio-sclerotic atrophy or chronic interstitial nephritis, or a combination of these changes. Above all, I object to the term "granular *contracting* kidney," which has absolutely no reason for existence, seeing that it is merely an assumption that the kidney is contracting. One is as much entitled to suppose that it is increasing in size, particularly as regenerative changes may frequently be seen in the epithelium of the tubules. Leading on from these remarks, I would urge the fuller recognition of arterio-sclerotic atrophy of the kidney as an individual condition which should be placed with the atrophies rather than with the nephrites. In the kidneys of old people, and occasionally also in younger persons, this change is sometimes seen, as a practically pure condition, unaccompanied by interstitial changes. I think this would assist in simplifying our diagnosis in the case of the small granular kidneys which are so often slumped together under the name of chronic interstitial nephritis. Nevertheless, one recognizes that a combination of arterio-sclerotic atrophy and chronic interstitial nephritis is one of the commonest met with in the *post-mortem* room. This is easily understood, if one remembers that the same toxin may cause both the arterio-sclerosis and the interstitial changes.

I agree with those who regard arterio-sclerosis and atheroma as distinct diseased conditions of arteries and refuse to accept the hybrid term "athero-sclerosis" introduced by our German colleagues. The term "subacute diffuse nephritis" presents some difficulty. It is a convenient term if always used to denote the uniform involvement of parenchymatous, glomerular, and interstitial elements throughout the kidney, with loss of or irregularity of the vascular markings, but I have frequently seen and heard it applied to kidneys which did not show these characters. It is very difficult to understand how the changes arise in the "subacute diffuse nephritis." Is the diffuse interstitial change secondary to and dependent on a primary diffuse parenchymatous nephritis, or does the original toxin act equally on the interstitial and parenchymatous elements? Should one connect the constantly occurring proliferation of the epithelium lining Bowman's capsule with the parenchymatous changes or with the glomerular and interstitial changes? I incline to the former.

I would next refer to the small kidney with distinctly thickened capsule which on stripping leaves the outer surface of the kidney finely granular. The thickened capsule is *not* always firmly adherent, yet microscopically there is increase of connective tissue along the interlobular vessels and capsular veins. I am not inclined to place this with Dr. Gaskell's "primary contracted kidney" on account of the thickening of the capsule, but place it amongst the chronic interstitial kidneys. The most strongly adherent capsules occur, in my experience, in kidneys which show more general increase of the interstitial tissue. To assist in diagnosing between the arterio-sclerotic kidney and the chronic interstitial I have been in the habit of paying special attention to the condition of the glomerulus and Bowman's capsule. When there is thickening of Bowman's capsule, forming a distinct fibrous zone round about the glomerulus, this change being associated with proliferative changes within the tuft, I regard the condition as chronic interstitial or

* The evidence on which these conclusions are based is given in my paper in the *Journal of Pathology and Bacteriology*, January, 1912.

glomerulo-interstitial. When there is simple atrophy and hyaline degeneration of the tuft which becomes adherent to the Bowman's capsule, this showing no thickening, I regard the condition as secondary to arterial changes. I would point out that thin paraffin sections are essential for the purpose. From examination of frozen sections only one frequently diagnoses an increase of connective tissue which does not exist. Frequently also a collapse and atrophy of the tubules gives an appearance of increase of connective tissue which is not existent, but simply depends on a large amount of connective tissue being concentrated in a given space as a result of that tubular collapse, and I object to the terms "contracted" or "contracting" being applied in such cases.

There is, next, the important question of the relation of different changes, as representing successive stages of the same process. One must admit the probability of this, although it is seldom demonstrable. Alterations appear very frequently not to progress beyond the stage in which the kidney is enlarged and of increased consistence, a stage which some denote as "subacute" and others as "chronic." I believe that "chronic" is the correct term for some of these forms of nephritis. I have, however, examined kidneys which appeared to connect up the acute glomerulo-interstitial nephritis, with coincident parenchymatous changes, the type of which is the post-scarlatinal kidney, or that which I have seen in cases of fulminant epidemic cerebro-spinal meningitis, with the contracted chronic interstitial nephritis, with localization of the connective tissue, principally along the lines of the interlobular vessels.

In two cases I have seen real, small, pale, chronic parenchymatous kidneys. Some authorities are inclined to doubt the existence of this kidney. I do not refer to the kidney with pale grey or yellow tubular areas. One of these kidneys, which I examined specially and which is figured in *Post-mortems and Morbid Anatomy*, showed very little increase of connective tissue, no distinct thickening of the arteries, and practically no adhesion of the capsule. There was considerable proliferation of the epithelial lining of Bowman's capsule, and there was an appearance in many of the tubules of marked regenerative changes. This explained, to my mind, the origin of the large, smooth prominences on the outer surface, some about 5 or 6 mm. in diameter, and suggested that the changes might be somewhat analogous to those regenerative changes which characterize the so-called "coarse cirrhosis" of the liver.

I have not time to consider further the relation of arterial changes to the interstitial changes, but would urge that it might lead to good results if one were systematically to follow out the changes in kidneys in syphilitic cases, or in cases in which ulcerative endocarditis has led to subacute changes. In these cases the date of the infection is known approximately and such cases might afford important data which would increase our knowledge of the processes which occur and the stages through which they pass.

In conclusion, I would urge the difficulty of properly explaining these pathological changes and correlating the clinical and *post-mortem* findings, unless there be closer co-operation between the clinician and the pathologist. If the functional changes be more minutely worked out during life, there is a greater possibility of discovering at the *post-mortem* examination the anatomical changes which correspond to them.

Mr. S. G. Longworth (District Asylum, Melton) said he would like to ask whether, in kidneys in which multiple small cyst formation was found in the cortex, this condition indicated the character of the underlying microscopic changes, or whether it was simply an advanced condition associated with the various kinds of kidney fibrosis?

Dr. H. de Carle Woodcock (Leeds) said: As a clinician—one of those who have been referred to by the pathologists with such kindly, complacent, and persistent pity—I would wish that the discussion had taken even a wider scope. Much has been said concerning the changes occurring in the heart, the liver, the spleen—changes, apparently, varying as the kidney structure varies. The change in the structure of the arterial wall especially has received attention, but not a word as to the changes in the contents of the blood vessels.

Dr. F. Craven Moore (Manchester) said: It is my purpose to draw attention to the deficiencies in our knowledge of such disorders of metabolism as may occur in association with destructive diseases of the kidneys, either primarily or secondarily, rather than to attempt to make them good. Apart from certain experimental investigations and isolated observations in cases of human nephritis, the great bulk of the extensive analytical and physico-chemical investigations in cases of nephritis has been concerned with the estimation of the secretory capacity of the damaged organ. The difficulties of metabolic investigations where the main excretory mechanism is at fault are very considerable, and the certain elimination of fallacies seems wellnigh impossible; for not only has the imperfect and inexplicably variable excretory capacity of the diseased kidneys to be reckoned with, but also the possibility of other organs, as the skin and alimentary mucosa, vicariously assuming an excretory rôle. In such cases the difficulty of assigning any particular result to a perversion of metabolism or to excretion is patent, and we must, I think, look rather to the appearance of products of incomplete nitrogen metabolism than to variations in the sum of metabolism for evidence of perversion. It has been assumed that a diminution in the amount of kidney substance may produce a disturbance of general metabolism quite apart from the impairment of the excretory capacity of the organ. This view was maintained by Rose Bradford, who found experimentally that the removal of not more than one-fourth of the total kidney weight was followed by great wasting and an increased excretion of urea. A repetition of these observations by Bainbridge and Beddard, on the other hand, led these observers to interpret the wasting and the increased urea excretion as being but the expression of starvation. It has been determined that in dogs bilateral nephrectomy proves more rapidly fatal than ligation of both ureters, and that the injection of either normal serum or renal juice into nephrectomized animals will prolong life even beyond that of animals in which only the ureters have been ligatured. When it is recalled that there is on record an authentic case where a human being survived some eleven days after the excision of a solitary kidney, it is difficult to feel satisfied with any deductions from these rapidly fatal experiments. Such observations as have been made on the human subject suffering from nephritis are curiously negative in evidence of any general disturbance of metabolism. In uncomplicated nephritis nutrition may be maintained with adequate dieting, and the oxygen intake and the carbon dioxide excretion remain within normal limits. Investigations of nitrogenous metabolism which have been sufficiently prolonged to eliminate the curious and unaccountable variations in nitrogenous excretion characteristic of nephritis have shown that nitrogenous balance is maintained—it may be for a time there is a loss, then for a time a gain, followed, it may be, for weeks together by a balance, but the sum of the whole is a balance, the variations excretory. What few observations have been made on the products of endogenous purin metabolism seem to indicate no abnormal relations. That a perversion of nitrogenous metabolism may obtain is seen in the relative increase of ammonia nitrogen to the total nitrogen; this is especially evident in uraemia, where it may attain 18 per cent. of the total nitrogen. Of the amido-acid nitrogen, although previous investigations have failed to demonstrate any relative increase, yet I believe that further research in this direction with the modern methods would be well worth the time consumed, certainly it would give us some clue to the functional capacity of the liver.

SOME FALLACIES IN THE ROUTINE TESTING OF URINE.

By Hugh Maclean, M.D.Aberd., M.Sc.Liverpool,
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The subject of fallacies in urine testing is a large one, but from the point of view of general medical practice the routine testing of urine in many cases practically resolves itself into an examination of this fluid for protein and

sugar. For practical purposes there is little doubt that the detection of these substances is of more material aid in diagnosis than any test for other pathological ingredients of urine. The detection of protein is comparatively simple, and in nearly every case a strict examination according to the methods suggested in the textbooks will settle the point. In the case of glycosuria, however, the matter stands on a different footing, and in this paper I propose to deal shortly with the subject of urine testing for sugar and the fallacies connected with it.

The great difficulty experienced in testing for sugar by the ordinary reagents is that of interpreting correctly the reaction obtained. The two tests in use in this country are Trommer's test and Fehling's test, and when sugar is present in abundance they are as satisfactory, if correctly applied, as any chemical test can be. It is otherwise, however, in the case of small amounts of sugar—here ambiguous reactions are obtained, the interpretation of which gives rise to much difficulty and confusion. The only help the textbooks give is summed up in the two words, "interfering substances," though how and why they interfere and what these substances exactly are is left to the imagination of the reader.

Trommer's Test.

Trommer's test has the advantage of simplicity; a solution of copper sulphate and sodium hydroxide are the only chemicals required. On the other hand, the test is somewhat difficult of application, and unless certain precautions are adopted quite erroneous results may be obtained. Any one can test this for himself by taking a urine of specific gravity of 1017 to 1018. Of this urine two portions of 5 c.cm. are taken, and to one is added first 1 c.cm. of a 10 per cent. solution of $CuSO_4$, and then half its volume of 15 per cent. sodium hydroxide; to the other the alkali is first added and the copper afterwards. In the case of the first urine, to which the copper was added before the alkali, it will be seen that a deep blue solution is formed, whereas in the other much of the cupric hydrate remains undissolved. On heating the blue solution a precipitate may be obtained, while the other mixture does not change. When this urine is tested by other means it is found to contain only the normal trace of sugar; it is therefore obvious that care must be taken in performing Trommer's test not to add the copper before the alkali. On the other hand, if excess of copper is added even after the alkali, a "sugar-free" urine will sometimes give a reaction. These results are attributed to the presence of creatinin by Schulz and by Salkowski. A solution of creatinin behaves in exactly the same way, as there is some evidence that this substance is the cause of the anomalous reaction. How it acts is not well understood. On the whole, Trommer's test is an unsatisfactory one from the clinical point of view, for it often gives rise to errors no matter how it is performed.

Fehling's Test.

Fehling's test depends, of course, on the same reactions as Trommer's test, only in this case the reagent itself contains the cupric hydroxide in solution. It is simpler than Trommer's test, and in practice a few cubic centimetres of the urine are boiled with an equal amount of the solution; if a red or yellow precipitate appears in a short time the test is positive and sugar is present. In many cases, however, it is very difficult indeed to decide whether or not a trace of sugar is present. On just heating the mixture of urine and Fehling's solution no change takes place, but after a minute or so it becomes opalescent and greenish in appearance, and if left to stand a slight precipitate may gradually settle out. In other cases nothing happens even after the urine is heated for a considerable time, but on standing a more or less well-marked precipitation takes place; again, after boiling for some time a dirty opalescent greenish milky liquid without any sign of a definite precipitate may be seen, or the solution may have merely changed to a greenish colour and all stages from a faint dirty greenish opalescence to a definite yellowish mixture exhibiting undoubted evidence of a precipitate may from time to time be observed in clinical work. The significance of these ambiguous reactions has not been sufficiently dealt with in the literature, and their explanation has never been seriously attempted. Some years ago I made some experiments on the subject, in order, if possible, to determine the factors on which these anomalous reactions depend, and the following explanation may help to make the matter more clear.

If we take a normal urine which gives no reaction with Fehling's solution when treated in the ordinary way, and add to this urine a small amount of sugar, it is often found that no evidence of the added sugar is indicated by this test. Since Fehling's solution is capable of detecting one part of glucose in 125,000 parts H_2O (0.008 per cent.), it is obvious that lack of delicacy is not the cause of the failure in this case. Since, therefore, small amounts of sugar capable of giving a distinct reaction in aqueous solution give no reaction when added to an equal amount of urine, it is obvious that urine must contain something which prevents small quantities of sugar from reacting normally with Fehling's solution. This substance is creatinin, which is normally present in urine in sufficient abundance to interfere with Fehling's test. Creatinin is capable of holding in solution the reduced suboxide of copper formed as the result of the action of sugar on the cupric salt, and so the mixture gives no evidence of a precipitate indicating the presence of sugar.

For this reason normal urine, which contains an appreciable amount of sugar, gives no reaction with Fehling's test. It will be seen that the action of creatinin is in this case more beneficial than otherwise, for it prevents mistakes being made with physiological urines containing only the normal amount of sugar.

The action of creatinin, however, does not end here; it further possesses the property of materially modifying the nature of the precipitate when there is a slight excess of sugar present. If, for instance, we take a 0.3 per cent. sugar solution, and to one portion of this add a few milligrams of creatinin, it will be found that the precipitate obtained on heating with Fehling's solution is distinctly yellow and finely divided; without the addition of creatinin a dense red granular precipitate separates out.

The explanation generally put forward is that the yellow precipitate consists of reduced cuprous hydrate, while the red one is composed of the reduced oxide. Cuprous hydrate, however, is so unstable that it is difficult to accept this explanation; again, it entirely fails to explain all the different shades between green and yellow so often observed. It is more probable that the colour of the precipitate formed depends on the fineness of the granules, for microscopic examination of these different coloured precipitates shows that some relationship exists between the colour of the precipitate and the relative size of the particles. In a urine which after boiling for some time gives a dirty greenish, opalescent solution, the modified colour is due to the fact that the precipitate of cuprous oxide is present in an exceedingly finely divided state. In the case of a greenish-yellow precipitate the particles are still very fine but rather coarser than in the last; with a yellow precipitate they are somewhat larger, and this increase in the size of the particles goes on until in the case of a distinct red precipitate the size of the individual granules is much more marked. It is probable that all the precipitates are really composed of cuprous oxide. The causation of these ambiguous reactions is easily explained in the light of the above observations. In examining a urine containing just the normal trace of sugar the equivalent amount of Fehling's solution is reduced in the ordinary way, but the creatinin present holds the reduced cuprous oxide in solution, so that no change is apparent to the eye. When the urine contains just a slight excess of sugar the creatinin present is not sufficient to hold the cuprous oxide in solution, but it modifies the nature of the precipitate, so that it separates out in an exceedingly finely divided form. These fine particles floating in the liquid give a dirty milky greenish appearance to the fluid. These ambiguous precipitates are just modifications of the normal form and nearly always indicate a slight excess of sugar above the amount present in normal urine. In concentrated urines these ambiguous reactions constantly occur without in any way indicating that the total amount of sugar excreted is at all in excess of the normal, though the amount per cubic centimetre of urine is naturally somewhat above the average.

Of course, it is obvious that the presence of a sufficient amount of some other reducing body—glycuronic acid—

would act in the same way, but observation shows that sugar is really the substance which most often gives these modified reactions.

Again, in certain urines it is only after boiling with Fehling's solution for some time that a result is obtained; this is due to the fact that though the creatinin holds in solution the cuprous oxide formed during the initial stages of boiling, yet the creatinin itself is gradually destroyed by the alkali, and a point is reached at which the creatinin present can hold no more cuprous oxide in solution; at this point a precipitate settles out.

Even normal urine, when boiled long enough with Fehling's solution, will give a distinct precipitate, so that Fehling's test is positive in every urine if the heating is sufficiently prolonged.

The following extract from one of my papers on the subject indicates shortly what happens in normal urine, and exactly the same mechanism is present in pathological urines with excess of sugar:

> The chief reducing substances present in all normal urines are uric acid, carbohydrate material (of which the larger part seems to be dextrose), and creatinin. Uric acid and sugar, however, differ greatly from creatinin in regard to the time required to cause reduction. Both substances, in the percentage in which they occur in normal urine, are capable of effecting reduction almost immediately on the boiling point being reached; in the process of reduction they undergo destruction.
>
> Creatinin, on the other hand, reduces very slowly indeed. When a normal urine is boiled with Fehling's solution, the uric acid and sugar present almost immediately reduce their equivalent amounts of the solution; no effect is perceived owing to the fact that the reduced suboxide is held in solution by the creatinin; after boiling for a very short time the full reducing effect of both acid and sugar is completed. On continued boiling the creatinin gradually causes further reduction, at the same time becoming gradually diminished in amount; part of it is probably converted into creatin while part is destroyed.
>
> Ultimately a point is reached at which the suboxide reduced by the sugar and uric acid, added to that reduced by the creatinin itself, is no longer capable of being held in solution by the amount of creatinin and its derivatives actually present in the urine at that moment; at this point precipitation occurs.
>
> Thus it will be seen that the reaction obtained from a normal urine is very similar in character to that obtained from a urine containing more than the normal amount of sugar, the chief difference being that the more sugar present the quicker the reaction occurs.
>
> In the presence of great excess of sugar the effect of creatinin is of course quite obscured and of no practical importance. It will be seen that the influence of creatinin on Fehling's solution is very marked and important, though somewhat different from that ascribed to it in the textbooks, where its action is generally considered in relation to its direct reducing power. This direct action occurs but slowly, and is therefore very insignificant when Fehling's test is used in the ordinary way; its direct inhibitory action, however, explains many points which are otherwise obscure, and for which no definite explanation has been forthcoming.

In general, therefore, it may be taken for granted that a urine which gives an ambiguous reaction with Fehling's solution contains a slight excess of sugar per c.cm. This may be due to concentration or other causes, and its significance must be estimated from a consideration of other details of the case. When certain drugs are being given similar reactions may, of course, occur.

It is hoped that these remarks will make clear the mechanism of anomalous reactions obtained in testing urine for sugar with Fehling's solution, and so render their interpretation more easy. In doubtful cases fermentation with yeast, formation of the osazones, and other more or less complicated tests must be applied.

On some Causes of Glycosuria.

With regard to the significance of a slight excess of sugar in the urine, it may be unimportant or of great importance. The facility with which certain factors produce glycosuria has hardly been sufficiently realized, and in this direction alcohol has a very pronounced effect. I once examined a number of urines from patients suffering from the effects of alcohol—most of them acute cases—and in almost every instance sugar was present. This led to further experiment, and it was found that in many cases the ingestion of comparatively small amounts of alcohol induced fairly well marked glycosuria; in some people really trifling amounts produce this effect. In the case of six temperate persons who had dined out, and whose urines I had an opportunity of examining, sugar in small amount was found in five of these. All the cases cleared up in a day or two.

Again, after certain drugs, such as $CHCl_3$, it is known that the urine reduces Fehling's solution, and this has been ascribed to the presence of glycuronic acid, but in many cases at least it is due to sugar, for nearly all these urines are fermentable.

These urines show only occasional sugar reactions, and the presence of sugar seems to be of little importance; but when a urine constantly gives even an ambiguous Fehling's reaction, and no cause such as drugs, alcohol, etc., can be found, it may be of grave significance as indicating the onset of diabetes.

DISCUSSION.

Dr. P. J. Cammidge (London) said: The many fallacies in connexion with Trommer's and Fehling's tests make it necessary that great care should be taken in interpreting the results. For clinical work I have for some time made extensive use of Benedict's test, which has the advantage of using a simple solution that keeps indefinitely, and requiring only a very small quantity of urine, and with very satisfactory results. Slight reactions for sugar are always important, and, unless it can be proved by a test meal of sugar that the cause is quite a temporary one, should raise a suspicion that the patient is a potential diabetic, and the diet should be worked out on the basis of the experimentally determined sugar tolerance.

Professor Walker Hall pointed out that in glucose test meals it was imperative to exclude the action of protein as a great irritant of the sugar-producing mechanism. He considered that it was advisable to diminish the intake of protein a little below the average on the days when the glucose was given.

Sir Bertrand Dawson (London) said: Dr. Maclean's paper throws further light on an important question. It is important to know whether unexpected reduction of Fehling's reaction is due to sugar or not in any given case, and it is interesting to note that in Dr. Maclean's experience in many cases the reducing substance is sugar. What is the significance of occasional glycosuria? In the first half of life it certainly does mean increased liability to diabetes, and can never be ignored. Sugar, if it appears after big and luxurious meals, whether you style it alimentary or not, denotes such liability. From the point of view of the medical adviser it should lead to insistence on caution always being exercised as regards carbohydrate food, especially sugar. From the point of view of the medical adviser to insurance companies it should lead to loading the premium or decline of the proposal. In any doubtful case it is wise to give a test meal of 100 grams of pure glucose and test the urine afterwards. The healthy body should be able to deal with that quantity of sugar without glycosuria appearing.

THE EXCRETORY FUNCTION OF THE INTESTINE AND ITS RELATION TO DISEASE.

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The concretions not infrequently found in the intestine and appendix are in their descriptions usually dismissed with a term such as faecal concretion, faecal accumulation, or enterolith. Yet, when they are carefully examined, they are often seen not to be of so simple a nature. The object of this communication is to show that their chemical composition, their structure, the time and site of their occurrence, reveal many common factors in them, in whatever part of the intestine or its appendages they may occur. A consideration of these factors points to a possible solution of their formation and suggests that they are manifestations of some abnormal condition in the excretion of the intestine and its appendages. I have previously[1] called attention to the close similarity in the chemical composition of the intestinal sand so frequently found in cases of mucous colitis, and of the concretions in

the appendix. In this note I describe and show concretions formed in the intestine itself which have closely allied composition; and call attention to certain conditions in the gall bladder which appear to suggest that gall stones may in some way be due to a similar anomaly of excretion on the part of the gall bladder.

Intestinal Sand.

Most of the published analyses of this material have referred mainly to its inorganic constituents, which are largely calcium compounds. In analysis I have published[2] it is shown that nearly half of the material consists of organic matter. Of the inorganic matter one-half is calcium. The total fatty acids constitute nearly one-fifth of the material. Half of the calcium present is present as calcium phosphate, the remaining half being present as a soap of saturated fatty acids.

Appendix Concretions.

I have shown in the same communication that appendix concretions are undoubtedly formed in the appendix itself by the successive deposition of layers of material formed by the mucous membrane lining its wall.

That the appendix has a secretion or excretion which, under abnormal conditions, could form these concretions is to be seen in any appendix in the *post-mortem* room. Sir William Macewen has demonstrated the presence of such a secretion during life. Schmidt[3] states that the intestinal mucous membranes excrete inorganic salts, such as those of iron, calcium, and phosphoric acids, and also fatty substances. Recently Mr. Paul has kindly supplied me with an appendix which he removed which shows this in a striking manner. The appendix is constricted at about its middle, and behind this constriction is, not a concretion, but a small collection of material which consists of mucus, a fair amount of fat compounds, and some calcium soaps. The specimen shows the material being extruded into the intestine. The amount was too small to allow of quantitative estimation. A case of Mr. Parker's illustrates a further degree of this same process. This appendix, removed from a man aged 41, is unusual in shape, and opens into the caecum by a very wide orifice. Distal to this orifice, behind a constriction, is a marked hypertrophy of the muscular layers. This extended for about half the length of the tube. Beyond was a dilated portion full of white pultaceous material. Some of the material was examined qualitatively and shown to contain calcium soaps and calcium carbonate, the latter apparently in fair quantity. The calcium carbonate was probably formed by the reduction of the calcium soaps.

As a result of these findings I have suggested that appendix concretions are due to the secretion or excretion into its lumen of a material rich in calcium soaps the fatty acid radicles of which are of the saturated type.

The close resemblance in chemical structure between appendix concretions and true intestinal sand, and the finding of intestinal sand so commonly in cases of mucous colitis, which clinically is frequently associated with disorders of the appendix, thus suggests a common factor in the etiology of these two conditions.

This factor I believe to be the abnormal excretion of these soaps and allied compounds by the mucous membrane. This view is further supported by the examination of other concretions, obtained from the intestine, which I have recently had the opportunity of examining.

Intestinal Concretions.

The commonest cause of intestinal obstruction produced by an extraneous body in the lumen of the gut is ascribed to the so-called faecal masses, the next commonest being gall stones, and rarest of all the concretions, calculi or enteroliths with which this note deals. Of 669 cases of intestinal obstruction at the London Hospital in thirteen years[4] 69 were attributed to faecal accumulation, 15 were due to gall stones and only 1 to a concretion.

As the work mentioned above shows that appendix concretions are not, as is usually thought, merely faecal accumulation, so the true intestinal concretions I describe tend to show that a chemical consideration of so-called faecal accumulations in the intestine may reveal that they are in more cases the result of some abnormal process of the intestinal mucosa and not a mere accumulation of faecal material.

The presence of cholesterin in the first intestinal concretion shown would suggest the possibility that some of the large so-called gall stones supposed to have ulcerated through into the intestine may have been formed or enlarged in the intestine itself.

The first intestinal concretion shown is from a patient suffering from acute intestinal obstruction. It was situated in the small intestine in the upper part of the jejunum. Recognizing the unusual character of the mass, Mr. Paul kindly sent it to me to examine. It is about the size of a small Tangerine orange. In the centre is a raisin, around which is a bright yellow pultaceous mass held together by a stroma of vegetable material. The exterior was stained by faecal material. It was found to contain 31.4 per cent. of fat—of this 74 per cent. was present as combined fat having the low iodine value of 18 per cent. The unsaponifiable material was isolated, and proved to be cholesterin. That this was not a concretion such as is sometimes formed by the administration of large quantities of olive oil was proved by the nature of the fat.

This concretion is obviously not a gall stone, and points to the deposition round a nucleus of material excreted from the wall of the intestine, the interesting feature being that it contains cholesterin.

The second intestinal concretion was kindly sent to me by Miss Ivens, who removed it from a female aged 18. The patient had always had, as she described it, a delicate stomach. There was no history of colitis, nor had she taken olive oil or saline purgatives. After a period of alternating diarrhoea and constipation, she suffered from pain, retching, and flatulence, and six weeks later vomiting. The mass, causing a partial obstruction of the intestine, was found in the lumen of the small intestine at the lower end of about 10 in. of hypertrophied gut, which was firmly contracted round it.

The concretion is about 4 in. in diameter, round, white, concentrically laminated, and built upon a nucleus probably of degenerated epithelium. The whole mass is soft and friable, and has a soapy feel and appearance. The dried material contains 33 per cent. of calcium, 25.6 per cent. of neutral fat, and 7.7 per cent. of combined fat. There is a fair amount of phosphate and oxalate, and traces of other inorganic salts.

The third specimen consists of a number of concretions passed per anum by a typical case of mucous colitis. They are oval, averaging about 1 in. in length, and consist almost entirely of neutral fat. Large numbers were passed over a period of some months. The patient had previously not taken any oil or fat in excessive quantity. The fat in the specimens is saturated. As bearing upon the deductions to be mentioned later, it may be stated that after the continuous administration of olive and cod-liver oils (unsaturated) these concretions ceased to appear, and the mucus disappeared from the stools, the patient gaining 20 lb. in weight in three months, with the relief of all symptoms.

The examination of the three specimens described in no way suggests that they are due to faecal accumulation. In neither of the first two cases was any note made of the sacculi so common with faecal accumulations. Their chemical composition further points to their having some other origin, and I would suggest that they, like intestinal sand and appendix concretions, are the product of an excretion from the intestinal mucosa.

Gall Stones.

The well-known work of Naunyn which attributes the formation of gall stones to a lithogenous catarrh needs no recapitulation.

The usual classification of gall stones into the single cholesterin stone and the multiple pigment stones is incomplete. The purest cholesterin stone always contains some pigment, while the common cholesterin stone invariably contains a good deal of pigment which is always present as a calcium salt, and, further, calcium carbonate is usually present, as it is in the outer layers of the specimen shown. This mixture is the usual finding in the common gall-bladder stones.

Naunyn and his pupils conclude that cholesterin is not a specific product of liver secretion, neither is it a product of ordinary metabolism. It is often considered a decomposition or cleavage product of protein, because of its frequent occurrence in necrotic tissues, such as atheromatous

abscesses, old infarcts, and necrotic areas, but it must be remembered that in these foci there is always marked fatty changes and often calcification.[5]

Work which Mr. Branch and I did on the fatty acids in cod-liver oil suggests the possibility of cholesterin being a cyclical compound formed by the condensation and rearrangement of fatty acid molecules.

With regard to the presence of calcium, the following points may be noted as showing that it is not necessarily due to any change in the bile itself.

An increased calcium content in the food does not under normal conditions increase the calcium content of the bile. When bile is concentrated to produce precipitation of the pigments they are deposited as free pigments, and not as calcium salts. The presence of calcium pigment stones is not associated with an excess of calcium in the bile. This evidence is against the formation of the cholesterin, the calcium and the fatty acids, from the bile, and therefore gall stones are probably the product of the bladder wall.

That the material formed in disorders of the gall bladder is not confined to the cholesterin or calcium salts of bile pigments is shown by the two following cases.

In a case of Mr. Paul's, a male, the symptoms suggested the presence of gall stones. Intestinal sand was found in the stools. When operated upon there were found in the bile passages and neighbouring portions of the intestines numerous very small brownish-red granules, but no stones. An analysis of these granules showed that they contained fat in large quantity.

In similar circumstances Sir B. G. A. Moynihan,[6] operating for gall stones, found in three cases not stones in the gall bladder, but a peculiar condition of the wall, which was studded and impregnated with a white deposit, which, from its appearance in the photographs shown, looks like the material described in Mr. Parker's appendix case quoted above.

Certainly in Mr. Paul's case, and possibly in Sir B. G. A. Moynihan's, there was a disorder of the gall bladder, where fatty acid compounds entirely replaced the usual cholesterin and pigment, which, as a rule, are in greater amount than the fat compounds.

In another case, which showed symptoms undoubtedly like those of gall stones, Mr. Paul did an exploratory operation, and no gall stones were found. Some time later, the symptoms still persisting, an examination of the stool was made. A large number of crystals were seen, which appeared to be those of cholesterin.

Most of the conditions I have described have been looked upon as inflammatory.

The formation of calcium and fatty acids, or soaps, or the further stage of reduction of these, calcium carbonate, is common, as we have shown, in atheroma, in caseating and calcifying glands[5]—processes of degeneration rather than of catarrh or of inflammation.

Intestinal concretions and intestinal sand occur in cases of colitis, and here again an inflammatory condition is supposed to throw out these calcium or fat compounds or mixtures of them, or even, as I show in one of the specimens, cholesterin. The form of colitis with which they are usually associated is not inflammatory. I show a mucous cast from the case mentioned above in which there was no evidence of any inflammatory condition, either clinically or by leucocytes in the stool, and yet when examined it is found to contain fat and crystals of inorganic salts.

I would suggest, therefore, that all the concretions I have described are due to an excretion of the mucosa of the intestine or its appendages. That the intestine has important excretory functions is well recognized. It is certainly the organ for the elimination of the heavy metals, and it is probable that calcium is excreted into the intestine. The presence of fatty acid compounds in all these concretions requires explanation. In the fat metabolism of the body the tissues utilize mainly unsaturated fat. Hitherto it has been accepted that all fats are completely combusted to carbon dioxide and water. But the presence of saturated fat compounds in degenerative processes, such as atheroma, necrotic glands, and in fatty degeneration of the cells and other conditions, suggests the possibility of saturated fatty acids being a burden on the animal economy. If this be so, is it not possible that the intestine is an excretory organ for the waste products of fat metabolism, as is the kidney for proteins, and the lungs for carbohydrates? Should this be a possible explanation, it is not difficult to recognize that under abnormal conditions of fat metabolism the intestine would be called upon to excrete these bodies, and such concretions as the above, containing saturated fat or insoluble calcium soaps, be so formed.

In the narrow lumen of the appendix these soaps, etc., would easily cause obstruction. I have shown that lesions can be produced without an actual concretion, by the deposition of the soaps in the mucosa and submucosa—so diminishing the vitality of the part and thus allowing of infection.

In the yet wider lumen of the biliary passages there is room for the onward flow and elimination of the material produced, and it is only when the process goes on a stage further and the mucous membrane produces allied bodies which are more easily precipitated that concretions are formed, though the two cases of Mr. Paul and Sir B. G. A. Moynihan suggest that symptoms may be produced even in the earlier stages. Is it not possible that these are the cases in which a cure is ascribed to olive oil, which as an unsaturated fat influences fat metabolism, and so minimizes the excretion of the abnormal compounds?

In the still wider lumen of the intestine there is ample room for the production and elimination of these bodies, and it is only the effects such as mucous colitis which are noted, but on rare occasions the process is so advanced and rapid that even here the concretions may be formed and produce obstruction.

I would therefore bring forward the view, tentatively, that all these disorders I have referred to are not due to local disease at their site of origin, but that they are the outcome of a general metabolic disorder, which throws upon the intestine or its appendages the onus of excreting deleterious products.

While I have confined myself mainly to pathological data there is not wanting further clinical evidence to show a relation between these various (apparently widely) different diseases of the intestinal tract.

In a study of the statistics of the disturbances in the gastro-hepatic-duodeno-pancreatic physiologic system occurring in the Mayo Clinic, W. C. MacCarty states: "The figures are sufficiently significant to keep in mind as possible evidence of the probable fact that the inflammatory process in the appendix causes disturbances in the bile passages directly or indirectly." Is it not possible that a common factor may be at the bottom of both conditions?

REFERENCES.

[1] Abnormal Fat Assimilation associated with some Diseases of the Intestine, BRITISH MEDICAL JOURNAL, July 27th, 1907. [2] Ibid. [3] Von Noorden's *Metabolism and Practical Medicine*, vol. ii, p. 211. [4] Barnard, Allbutt and Rolleston's *System of Medicine*, 1908, vol. iii, p. 742. [5] Moore, Williams, and Joseph, Calcification in Glands and Arteries, *Liverpool Med.-Chir. Journ.* [6] *Annals of Surgery.*

DISCUSSION.

Dr. CAMMIDGE (London) said the excretory functions of the intestine were probably much more important than was generally supposed, and it appeared probable that the heavy metals and fats were so excreted. He had found that in colitis the saponified fats and inorganic oil were nearly always increased, and in some forms of metabolic disturbance due to disease of the ductless glands, apart from the pancreas, the same conditions appeared to hold good.

THE INVESTIGATION OF PUNCTURE FLUIDS AS AN AID TO DIAGNOSIS AND TREATMENT.

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THE advances made in medicine during the past twenty years have been almost entirely due to the special scrutiny of materials derived from the patient rather than by examination of the patient himself. Foremost, probably, amongst these materials are to be reckoned certain fluids obtained from the body by puncture, the examination of which gives us extra data essential for accurate diagnosis. No group of investigations in clinical pathology yields greater possibilities of help than this group, and I am

framing my remarks to-day largely with a view of stimulating the profession to more frequent application of these possibilities.

Technique.—This is all-important, and at the risk of being thought unduly rudimentary I shall refer to it in some detail. Like all methods of examination, the success of these punctures depends very much upon the ease and skill with which they are performed. Even the prick which is necessary for a blood count—which, by the way, is really the investigation of a puncture fluid—even this can be done clumsily and painfully, as many a patient knows to his cost. A man once told me that the only thing which stood out clearly in his memory after a prolonged time of delirium in typhoid fever was being roused by the efforts of his doctor to extract blood from his hand by the repeated and ineffectual jabs of a large needle. What shall I say of less simple procedures? Of the brutal trocars used not infrequently to puncture the chest, screwing up the skin, and bursting through it by sheer force, rather than by making a sharp, clean wound, and of other atrocities?

Quite often the suggestion that a puncture is advisable in the interests of diagnosis and of treatment is met by pleas of avoiding pain and discomfort; it is therefore the duty of the practitioner to disabuse the minds of the patient and his friends on this score by surprising them with prompt and skilful manipulations. And to this end no amount of care in the preparation of the needle and its adjuncts is wasted. Given such care, puncture of almost any region of the body should be comparatively painless. The two great essentials to successful puncture are sharpness and cleanness of the needle used—sharpness judged by looking at the point of the needle, if in doubt, by a lens; and cleanness, not only from bacterial contaminations by boiling, but from traces of rust. Even a little rust on a needle causes pain.

A simple, but effectual, method of avoiding this is to rub the needle up and down the fine brown emery powder which is found on a box of safety matches before it is boiled. Needles should always be provided in duplicate, and should be kept in spirit when not in use. They should not be used too many times; needles are cheap and a doctor's reputation for skill is dear. If a fine needle can be used no anaesthetic is necessary. But in all cases where a needle of larger bore is used, or where, as in pleural or joint or lumbar puncture, the length of the needle necessitates its being fairly stout, the preliminary use of a local anaesthetic is advisable. A few drops of novocain are injected by a very fine needle beneath the skin of the puncture area, and the operator waits until the skin is insensitive. A small incision is then made by a sharp thrust of a scalpel, and the puncture needle is then pushed steadily to the required depth. There are two classes of patients in whom a general anaesthetic is advisable—in children (not infants) who cannot understand that their co-operation is necessary, and in adult patients who are delirious. By giving a general anaesthetic in these cases sudden changes in the position of the body are avoided during the procedure.

The various punctures which from time to time are indicated as affording help in diagnosis or treatment fall into three groups:

I. *Puncture of Certain Cavities.*
- (*a*) Pleural sac.
- (*b*) Pericardial sac.
- (*c*) Peritoneal sac.
- (*d*) Spinal theca; lateral ventricles.
- (*e*) Joints; bursae.

II. *Puncture of Certain Solid Organs.*
- (*a*) The lung.
- (*b*) The spleen.
- (*c*) The liver.
- (*d*) Lymph glands.

III. *Puncture of Certain Adventitious Structures.*
- (*a*) Abscesses.
- (*b*) Cysts.
- (*c*) Growths.

Of the dangers or ill effects of these punctures I have little to say, because I hold that when performed with care and discretion, these are practically non-existent. Personally I always regard the business seriously, that is, I do not carry out such things as pleural or lumbar punctures in the out-patient or consulting room, because I am not prepared to run such risks. The patient is in bed, and remains there for at least twenty-four hours afterwards. I believe this reduces the after effects to a minimum, and, if one of the very rare disasters seen after a puncture should occur, it is obvious that it has not occurred because of any lack of proper precautions. Headache of temporary duration, and on one occasion a herpetic eruption, are all the ill effects I have seen following lumbar puncture; but the patient who developed herpes was suffering from early general paralysis of the insane.

If too much cerebro-spinal fluid is removed, however, the headache may be intense and prolonged, and a clumsy lumbar puncture may be followed by much local pain. Sudden or rapid death, as by vagus inhibition, has followed puncture of the chest wall in a few rare instances. When, a few years ago, I advocated lung puncture as a valuable aid to diagnosis in certain cases, it was urged that the proceeding was dangerous, owing to this last-named event. But this criticism takes no stock of the fact that negative pleural punctures are performed in thousands of cases every year, and these must all be in reality lung punctures. Yet in deciding to puncture the chest, to settle the question of the presence of a pleural effusion, we do not consider we are subjecting the patient to the risk of sudden death. Puncture of the pericardial sac should probably never be performed unless the diagnosis between dilated heart and pericardial effusion is quite clearly made out. And puncture of the liver is by some authorities regarded as attended by risks which are avoided by incision.

The fluid obtained by either of the above punctures is subjected to four methods of investigation:

1. Cytological.
2. Chemical.
3. Physical.
4. Bacteriological.

These methods constitute the complete examination; one or other of them is usually of special importance, according to the nature of the case under investigation.

The chief use of puncture methods is to assist diagnosis. Time prevents my specifying with any completeness the variety of ways in which this assistance is given. I will refer to a few. (1) The cytological examination of pleural fluid enabled Widal and Ravant to enunciate a formula by which, with a very high degree of probability, an effusion is known to be of tuberculous or of pyogenetic origin. A marked preponderance of lymphocytes is in favour of tuberculosis, whereas a marked preponderance of polymorphonuclear cells is in favour of pyogenetic infection. With somewhat less certainty the formula may also be applied to other than pleural exudates. (2) Chemical examination of puncture fluids from serous sacs enables us to judge to what degree they are transudates or exudates. The cerebro-spinal fluid obtained by lumbar puncture in cases of parasyphilitic disease is often shown to contain a globulin, demonstrable by the ammonium-sulphate test. Excess of urea in this fluid is, perhaps, one of the surest signs of renal inadequacy. (3) The help derived from bacteriological investigations of puncture fluids is everywhere apparent; the demonstration of the causal microbe in meningitis, in arthritis, in pleural effusion, and in pneumonia, is an everyday occurrence, when by no other method could it possibly be undertaken. These are some of the specific points that are capable of elucidation. In not a few instances the very existence of the disease is determined by the puncture, when without it the diagnosis is a matter of probability at most. The best instance of this is the diagnosis of meningitis. Several of the symptoms of meningitis are by no means pathognomonic, and are occasionally seen in the toxaemia of typhoid fever, pneumonia, septicaemia, and middle-ear disease. On the other hand, the less equivocal signs of meningitis are frequently absent early in the disease, when diagnosis is of paramount importance. From the state of ambiguity and speculation in which the practitioner is placed he is able, again and again, to arrive at a certain diagnosis by means of lumbar puncture.

The first step in successful treatment is an accurate diagnosis, and this, as has been seen, is much assisted by

the investigation of puncture fluids. The puncture itself is by no means devoid of therapeutic value at times. The drainage of a pleural effusion and of an ascites has been known as a means of treatment since the earliest days of medicine. Repeated lumbar punctures are certainly of benefit in certain cases of meningitis, whether due to the meningococcus or to other infections. Pyaemic joints may not seldom be saved from free incision by judicious aspiration, and the same applies to some forms of tuberculous abscess. A patch of bronchopneumonia will occasionally resolve, and the disease comes to a sudden end a few hours after a lung puncture. Considerations of this kind, however, scarcely come within the scope of our discussion.

DISCUSSION.

Dr. Purves Stewart agreed with practically everything Dr. Horder had said with regard to the types of cell found in leucocytosis of the cerebro-spinal fluid. Whilst he agreed that pyogenic infections usually produced a polymorphous leucocytosis, whereas tuberculous infection usually produced a monomorph leucocytosis, this rule was not absolute, for, during convalescence from a pyogenic infection of the meninges, the cerebro-spinal fluid passed through a later stage of monomorph leucocytosis on the way to recovery. Further, during an acute attack of tuberculous meningitis, the leucocytosis was not infrequently of a polymorph type. He therefore believed that it was not the particular organism which determined the type of the leucocytosis, but the acuteness of the infection—an acute infection producing the polymorph, a subacute or chronic producing the monomorph leucocytosis. Moreover, even an infection was not essential for the production of leucocytosis. He himself had experimentally injected the spinal theca in monkeys with a sterile emulsion of carmine when studying the lymph paths within the spinal cord. In every case there was produced a temporary polymorph leucocytosis, subsiding within a few days to a monomorph leucocytosis, and ultimately clearing up entirely. The speaker had never found it necessary to give a general anaesthetic in order to perform a thecal puncture, even in wildly excited or violent patients. He described a method whereby one could perform thecal puncture even on a violent patient by fixing his knees to his chin by means of a roller-towel or a strong portmanteau-strap.

Dr. Theodore Shennan (Edinburgh) congratulated Dr. Horder on his paper, but wished he had continued to give more details from his wide experience. He suggested keeping needles in a solution of 1 part lysol in 12 parts of dehydrated spirit, having found that by this means rusting of the needles was avoided. He confirmed Dr. Stewart's remarks as to the occasional occurrence of polymorph leucocytes in tuberculous fluids, particularly in pleuritic conditions. He asked for details as to the preparation, fixation, and staining of films from puncture fluids.

Dr. Horder, in reply, said: I quite agree with Dr. Purves Stewart that in some cases of tuberculous meningitis the cell content of the cerebro-spinal fluid is largely polymorphous, and not lymphocytic. These are usually rather acute cases. I have described, on the other hand, cases of meningococcal meningitis in which the cell content is largely lymphocytic, and not polymorphous. These were chronic cases. This gives a clue to the apparent variations from the formula which I quoted: the nature of the cell exudate is determined by the rate of its formation rather than by the nature of the infection. The formula must be taken as a general rule, and not as an invariable fact. Dr. Bernstein will find a very useful summary of the cell changes in cerebro-spinal fluid of acute anterior poliomyelitis in Dr. Mervyn Gordon's contribution to Dr. Reece's recent report to the Local Government Board on this disease. It is there shown that a moderate lymphocytosis exists in this condition, but that, if a careful consideration be given to all, and not one, of the tests applied to the fluid, it is not difficult to differentiate between the disease and meningitis.

THE ASSOCIATION OF CHRONIC DUODENAL ULCER WITH MORBID CONDITIONS OF THE ILEUM, APPENDIX, AND COLON.*

By D. P. D. Wilkie, M.Ch., F.R.C.S.,
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During the last two years a considerable amount of evidence has been brought forward, chiefly by operating surgeons, regarding the association of chronic duodenal ulcer with some lesion lower down in the alimentary tract. Thus, MacCarty and McGrath, Moynihan, and Paterson have noted the frequent coexistence of duodenal ulcer and chronic inflammatory mischief in the appendix; whilst Lane has found a tacking down, and kinking of, the lower end of the ileum in his duodenal ulcer cases. Before any far-reaching conclusions can be drawn as to the exact etiological relationship between these morbid conditions a large number of careful observations must be made both in the operating theatre and in the *post-mortem* room.

During the past year I have sought in the *post-mortem* room for evidence on this question, and in this time have encountered ten cases of duodenal ulcer. It is of interest that in only two of these cases had the duodenal trouble given rise to symptoms sufficiently clear to lead to a diagnosis during life; and in eight of these cases death was due to some other disease. The important point, however, was that in every case there were unmistakable signs of old inflammatory mischief in the lower bowel, and in most of them this was the outstanding feature in the examination of the abdomen.

The cases may be divided into two groups, first, those in which disease in the appendix was found—five cases belonged to this group; and, secondly, those in which the the appendix was healthy but the colon showed signs of old inflammatory mischief and was crippled by adhesions.

Group I.—*Appendicitis and Duodenal Ulcer.*

Case I.—This was a man, aged 26, who died after operation for perforative appendicitis and general peritonitis. He had suffered from recurring attacks of appendicitis—two or three in the year—for ten years before the final and fatal illness. At the *post-mortem* examination a typical scar was seen on the anterior wall of the duodenum, ½ in. from the pylorus. The fibrous contraction round the ulcer had resulted in a moderate stenosis of the duodenum at this point. On opening the duodenum two ulcers were found, one on the anterior wall, the second on the posterior wall just opposite. The former was represented by a shallow depression with a curious glazed surface; it had evidently almost healed over; the latter was a typical punched-out chronic ulcer.

Case II.—This was a young man, aged 21, who died six days after operation for general peritonitis secondary to gangrenous appendicitis. He gave a history of pain in the abdomen for fourteen days before admission to hospital, but of no previous

Fig. 1.—Case II, Group I.

abdominal trouble. A history was very difficult to obtain, however, on account of his toxaemic condition. At the *post-mortem* examination a chronic duodenal ulcer, the size of a threepenny

* These observations were made in the Pathological Department of the Edinburgh Royal Infirmary, and I wish to express my indebtedness to the staff of that department for their courtesy in allowing me to examine the cases.

piece was found on the posterior wall of the first part of the duodenum ¾ in. from the pylorus, and in addition there were numerous acute haemorrhagic erosions in the stomach.

CASE III.—This was a boy, aged 16, who died from toxaemia four weeks after operation for general peritonitis secondary to acute appendicitis. At the *post-mortem* examination several loculated abscesses between adherent coils of intestine were found, and in addition a portion of omentum was seen to be fixed by old-standing adhesions to a scar on the anterior surface of the first part of the duodenum. Microscopic sections of the duodenal wall at this level showed that there was no active ulceration, but merely the fibrosis associated with a healed ulcer.

CASE IV was that of an old man who died of mediastinal lymphosarcoma. Two chronic "kissing" ulcers were found

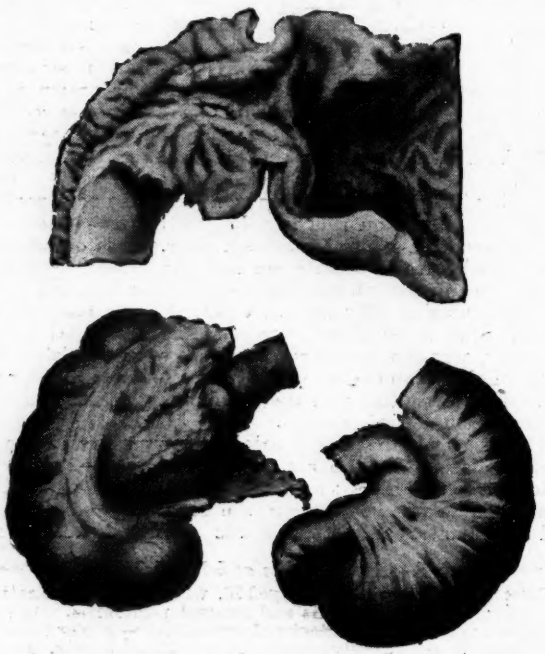

Fig. 2.—Case IV, Group I.

in the first part of the duodenum; in the lower part of the abdomen several coils of ileum were bound by old adhesions above which the small intestine was somewhat distended. The appendix was represented by a small stump ½ in. long and by two small nodules attached to the meso-appendix, but entirely separate from the stump. There had clearly been a former attack of appendicitis, with sloughing and spontaneous amputation of the appendix. In the history of his previous health it was noted that for six or seven years he had suffered from periodic attacks of indigestion and abdominal pain, and at one time had passed tarry stools, but his mediastinal trouble had quite overshadowed this.

CASE V.—This was a man, aged 62, who died from peritonitis due to the perforation of a large duodenal ulcer at the upper border of the first part of the duodenum, just beyond the pylorus. The lower end of the ileum was seen to be bound down to the brim of the pelvis and to the inner aspect of the caecum by old adhesions, on freeing which the appendix was disclosed. It showed clear signs of chronic inflammatory trouble, its distal end was enlarged and club-shaped, and the tip was not only adherent to, but was firmly incorporated with, the wall of the ileum.

In these five cases the coexistence of duodenal and appendicular disease was highly suggestive of some interrelationship.

GROUP II.—*Duodenal Ulcer and Pericolitis.*

In the remaining five cases of ulcer the appendix showed no visible pathological change, but the outstanding feature in the examination of the abdomen in each case was the marked degree of pericolitis present.

CASE VI.—This patient, a man, was admitted to hospital suffering from advanced general peritonitis, and gave a history suggesting duodenal ulcer; the abdomen was opened and a duodenal ulcer, typical but without perforation, was found. The appendix was not at fault, though there were old adhesions in the caecal region, and accordingly the abdomen was simply drained. Death ensued on the following day, and at the *post-mortem* examination there were discovered the signs of an old suppurative perityphlitic process which had lit up and ruptured into the peritoneal cavity, leading to general suppurative peritonitis. There was a chronic oval ulcer on the anterior wall of the first part of the duodenum ½ in. from the pylorus, and the puckered cicatrix of an old healed ulcer on the posterior wall just opposite.

CASE VII.—This was a man aged 56, in whom death had followed a late operation for a perforated duodenal ulcer. A typical history of five years of intermittent pain had been obtained. A perforation the size of a threepenny-piece on the upper border of the first portion of the duodenum had been closed and a gastro-enterostomy performed. At the *post-mortem* examination, apart from the duodenal trouble, attention was at once directed to the ascending colon, which was firmly bound to the parietal peritoneum of the right flank by dense adhesions; the omentum was caught up in these, and on separating it old-standing fibrous cicatrices in the wall of the colon were revealed. Viewed from the mucous aspect, the colon showed merely puckering at the site of the cicatrices. The caecum was considerably dilated above this, but the appendix showed no abnormality.

CASE VIII.—This was an old man who died from chronic Bright's disease. In this case a stellate scar on the anterior wall of the duodenum, to which a fringe of omentum was adherent, marked the site of a chronic ulcer which from the inner aspect was seen to have almost healed over. There was well-marked pericolitis of the descending and pelvic portions of the colon, both of which parts were thickened, bound down by adhesions, and in part enveloped by adherent omentum. The colon above this point was dilated and showed evidence of pronounced faecal stasis. The appendix was healthy.

CASE IX was that of a man, aged 22, who died from pneumonia. At the *post-mortem* examination a dense fibrous stenosis of the duodenum half an inch from the pylorus was seen. This corresponded to a typical punched-out chronic ulcer on the anterior duodenal wall. There was also very pronounced chronic pericolitis, the transverse colon being bound to the descending colon for about 12 in. by a mass of adhesions and giving the typical "double-barrelled shot-gun" appearance. The caecum, ascending and first part of the transverse colon were all distended with gas and scybalous faeces; the pelvic colon was collapsed. On the left side of the abdomen, alongside the descending colon, several coils of small intestine were loosely bound together by old fibrous adhesions. In this case

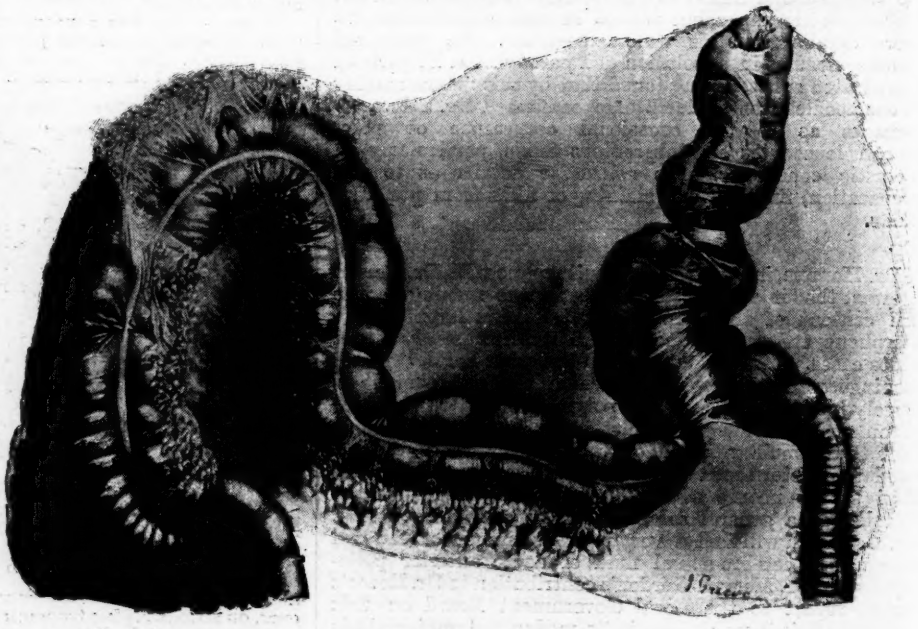

Fig. 3.—Case IX, Group II.

there had undoubtedly been a marked degree of chronic stasis in the colon.

CASE X was a man aged 62, who had died of aortic and mitral disease. In the history of the case it was noted that he had

suffered from a severe attack of "cramp in the stomach" eight years before his fatal illness. At the *post-mortem* examination dense adhesions concealed the first portion of the duodenum, and on separating these the scar of a healed ulcer was found on the upper border of the duodenum, half an inch from the pylorus. In this case the transverse colon in its middle third was bound to the root of the mesentery of the small

Fig. 4.—Case IX, Group II.

intestine by old-standing adhesions which anchored the colon firmly. The appendix was healthy, but the last three inches of ileum were fixed to the pelvic brim and to the front of the sacrum by what appeared to be old inflammatory adhesions which enveloped and constricted the ileum at this point, the intestine above being slightly dilated and hypertrophied.

In all these five cases a duodenal ulcer was associated with evidences of old-standing inflammatory trouble in the colon.

These *post-mortem* findings go to swell the evidence we already possess that duodenal ulcer is usually associated with some trouble lower down in the alimentary tract. It would appear that, whatever be the primary cause of the ulcer, a chronic intoxication from an infective lesion, or merely from faecal stasis, is the factor determining the chronicity or periodicity of the duodenal trouble. They indicate that at operations for the relief of duodenal ulcer a search should always be made for such lower bowel lesions; and that, where possible, these should be treated as being essential elements in producing the symptom-complex for which the operation was undertaken.

THE ETIOLOGY OF DISSECTING ANEURYSM.

BY

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The commonly accepted explanation of dissecting aneurysm is that it originates in a splitting of the endothelium covering a softened atheromatous patch, or over an atheromatous plate, followed by dissection of the blood into and along the media, with rupture sooner or later to the exterior of the aorta.

We wish to deal with a group of dissecting aneurysms in which there is no atheroma and no gross changes in the intima, and in which the important pathological changes affect the middle coat. Such cases appear to be sufficiently numerous to merit detailed consideration. In most of the cases we have examined the rupture of the intima has taken place in the ascending part of the aorta, a short distance above its origin. In a few cases rupture occurs at the commencement of the ascending aorta near the attachment of the obliterated ductus arteriosus, and occasionally it takes place in the descending aorta.

The last variety is difficult of explanation, and may sometimes depend upon the rupture of an intercostal or lumbar artery. Seeing that we have not had any opportunity of examining cases of this kind, we shall confine our remarks to dissecting aneurysms originating either in the ascending aorta or at the commencement of the descending aorta.

The explanations advanced by different writers have been classed under three headings: (1) Increased blood pressure due to hypertrophy of the left ventricle; (2) actual trauma; (3) degeneration of the wall. Seeing that several of our cases have shown no hypertrophy of the left ventricle, and that similar cases have been reported, we consider that any satisfactory explanation must disregard this factor, and account for the rupture in absence of any great increase in the blood pressure.

Actual trauma or severe unusual strain may also be excluded in many cases, so that this factor can be admitted only with the reservation that though often present it is not absolutely essential. The remaining factor—degeneration of the wall—is, we hold, the important factor in the causation of dissecting aneurysms.

Before dealing with the pathological changes which occur we may consider shortly the forces to which the wall of the aorta has to offer resistance. First, there is the *resistance to expansion*, which must be greatest where the diameter of the tube is greatest—that is, in the ascending portion.

Secondly, there is the *recoil* of the blood during diastole, which we presume has an effect comparable to that of the water hammer. This force will be greatest also at the commencement of the aorta. Thirdly, there is what we may term a *vibrating motion* in the wall, between the greatest expansion of the tube and its greatest contraction. This effect will be greatest also near the commencement of the aorta, and where the vibration is greatest there will be a great tendency to degeneration.

The principal agent which Nature provides to resist these forces is the elastic tissue of the media, of which the fibres and laminae are arranged transversely and obliquely. These laminae give the effect of a tube composed of multiple layers, and the stress upon these must increase as one passes outwards in the wall. Between the laminae are the muscular fibres and connective tissue, the latter close to, and, as it were, clothing the elastic tissue. These two elements may act like check cords, the connective tissue checking over-extension and the tonic contraction of the muscular tissue allowing the strain to come gently upon the elastic tissue.

Elastic tissue in either real or premature old age tends to undergo changes which render it more brittle; and if the other two elements also undergo degeneration, as is constantly seen in these cases, the effect is that the media will more readily give way even under a strain which under ordinary circumstances could not be regarded as pathological.

The principal changes we describe affect the middle third or junction of the middle and outer thirds of the media. This is the position in which engineers note degenerations of tubes of uniform structure—and conceivably this may be applicable in the case of the aorta—but a more important reason for this localization is that this is the area which is farthest removed from its nutritive supply, whether from the vasa vasorum or from the lumen of the vessel.

The changes we have noticed are fatty degeneration of the connective tissue closely apposed to the elastic laminae; increase of the connective tissue between the laminae, and its hyaline degeneration; thinning or varicose thickening of the elastic fibres with rupture of these, and sometimes rupture of numerous adjacent fibres leading to formation of "faults" in the media; atrophy and disappearance of the muscle fibres sometimes over a considerable area in the middle and also in the outer part of the wall.

In the intima of the normal aorta there may a distinct elastic lamina about its middle part. There are, in addition, numerous fine elastic fibrils embedded in its connective tissue, and, further, the inner strands of the elastic tissue of the media are condensed so as to form an inner elastic lamina. In the neighbourhood of the ruptures we have found that these layers of elastic tissue, as well as the fine fibrils in the intima, become teased out, and to a great extent disappear. There is fatty degeneration disposed in lines parallel to the lumen, and this is at times very marked in the neighbouring, innermost, layers of the media.

With the occurrence of these important changes in the media and in the intima it is easy to understand that when the media gives way the intima will tend to bulge into the hollow thus produced, and readily ruptures.

The dissection along the media at the junction of the middle and outer thirds appears to depend mainly upon the degenerative changes occurring in that part, and also on the fact that the outer layers of elastic tissue in the outer third of the media are frequently closer together than normal, as it were condensed, so as to afford greater resistance.

In many cases the aneurysm after progressing along the

media ruptures back into the lumen. This may occur in the iliac vessels or even in the femorals. This result must be due partly to the fact that the pressure within the aorta is reduced more quickly than that within the aneurysm, but, as we have not yet been able to demonstrate any excessive degenerative changes in the intima at the point of re-entrance, we are not able to afford a complete explanation as to why it should rupture internally and not externally.

Most of the aneurysms rupture externally at the commencement of the aorta, usually into the pericardium. This must be because the outer layers of the media and the adventitia have to withstand not only the original pressure within the aorta, but an increased pressure within the sac of the aneurysm, the height of the pressure depending upon the size of the rupture.

[The principal changes were demonstrated by means of the lantern.]

THE RÔLE OF THE GASTRIC JUICE IN THE PATHOLOGY OF GASTRIC ULCER.

(With Lantern Demonstration.)

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THE part played by the gastric juice in producing and maintaining gastric ulceration is a useful subject for discussion, since a knowledge of this aspect of the question is particularly useful to the practitioner of medicine in the treatment of the disease.

It is quite impossible to obtain an exact idea of the influence of the gastric juice in this connexion by the study of human pathology alone, and the diversity of the opinions which have been expressed at various times is a proof of this statement. The chief reason for this is that in any given case of gastric ulcer the pathological processes present involve so many factors that it is quite impossible to assign to each its proper significance and value. By experiments on animals we can isolate these factors and study the influence of each separately.

I propose, therefore, to give you very briefly the results of an experimental inquiry into this question, which have been published separately at intervals during the past few years.

The method by which I have produced these experimental ulcers in the first instance is one which I described some years ago, and consists in the injection into the animal of a toxic serum which has the property of attacking its gastric mucous membrane and producing gastric ulcers. Whether the serum reaches the stomach through the general blood stream or is injected directly between the coats of the stomach does not matter. In either case ulceration is produced, but in the former general toxic symptoms result, and the animal may die, whilst in the latter only slight symptoms result, and an ulcer is formed at the site of injection. The method of injection into the stomach wall is, therefore, the most convenient to employ when one is studying the healing of the ulcers, because an ulcer can be produced in any position in the stomach, and of any size, without the animal suffering more than a few hours' inconvenience from the general toxic effects of the serum. The serum is that of an animal which has been immunized by repeated injections of the gastric cells of another animal.

The two animals in which ulcers have been produced in these experiments are the guinea-pig and the cat—in the former by injection of immune serum obtained from the rabbit, and in the latter by injection of serum obtained from the goat. The lesions produced by this serum are necrotic in character. The necrosis involves the mucous membrane, and the result is an ulcer extending to various depths. It may only involve the submucous tissue, or may reach to the peritoneum, and perforation result.

The part played by gastric juice in the formation of these ulcers I have found to be a very important one. In fact, if the gastric juice be temporarily put out of action, ulceration fails to appear at all, although the animal dies from the general poisonous effects of the serum.

In order to demonstrate this, two guinea-pigs of the same weight are injected via the peritoneum with the same dose of serum. A solution of sodium bicarbonate is introduced into the stomach of one of them in order to put the gastric juice out of action whilst the poison is acting. If a lethal dose of the serum has been injected both animals will die within twelve hours. The stomach contents of the animal which has received the soda will be alkaline in reaction, and its mucous membrane be quite normal to the naked eye, whilst the stomach contents of the other will be acid in reaction, and the mucous membrane will show the usual black patches of necrosis and ulcers. Hence the serum does not produce the necrosis of the gastric cells, but brings about some change in them which renders them susceptible to the action of the gastric juice. The process is, therefore, one of digestion of the gastric mucous membrane by the gastric secretion owing to some change which has occurred in it. What that change is I cannot say, because no microscopic alteration can be seen in the cells before they are digested.

It does not appear to me to be due to the removal of any specific resisting power possessed by the gastric cells, because I have shown that other blood poisons, which cannot have any influence in this direction, are able to likewise bring about self-digestion. It is undoubtedly a question of the infliction of a certain amount of injury upon the cells, and necrosis and ulceration are produced merely because the cells happen to be exposed to the action of a digestive fluid.

The establishment of this principle is of great importance, because it demonstrates how blood poisons, of which we are at present ignorant, may produce their effects upon the stomach. We know that one important cause of gastric ulcer in the human being is bacterial invasion of the body either as a septicaemia or from some local, and perhaps unsuspected, focus of infection. The organisms, or their poisons, gain entrance to the gastric mucous membrane by the blood stream. It is not necessary for them to kill the gastric cells, but if a certain amount of damage is inflicted upon the latter, the gastric juice is then able to complete the death of the tissue and so produce an ulcer. This explains why these bacterial ulcers are found so much more often in the stomach than in other parts of the body. The same remarks apply to the ulcers found in other intoxications of the body—as, for example, in burns. In almost half the cases such toxic ulcers are single, and many instances are on record of the transformation of such acute ulcers into chronic ones.

This action of poisons circulating in the blood suggests that certain substances introduced into the cavity of the stomach may produce a similar effect, and we already have one protoplasmic poison present in the gastric juice—namely, hydrochloric acid. The effects of hyperacidity of the gastric juice are interesting. The amount of HCl present in the guinea-pig's stomach contents is the same as in man—namely, about 0.2 per cent. One may introduce into the guinea-pig's stomach solutions of HCl of any strength up to 0.7 and 0.9 per cent., depending on the weight of the animal, and no lesion is produced, but with strengths above these necrosis and ulceration are produced. In the human being the percentage of HCl never reaches these limits, and therefore hyperacidity alone, such as is found in disease, is unable to produce ulcer, but I have been able to show that it is *potentially* able to do so, and that in the presence of another poison a hyperacid gastric juice will do more damage to the stomach wall than one of normal acidity. In order to demonstrate this two guinea-pigs of the same weight are injected via the peritoneum with the same dose of serum. A solution of HCl, of any strength beween 0.3 and 0.7 per cent.—in other words an innocuous fluid—is introduced into the stomach of one of them at the same time.

In the twenty-two experiments of this series which I have done the result was in all cases the same. The gastric lesions in the stomach of the animal which had received the HCl were much more marked than those in that of the control animal. So that HCl, of such strengths as may be found in the condition of hyperacidity in disease, is able to act as a poison for the gastric cells. I have found precisely the same results to occur in the case of other acids, including biliary acids. The results with acetic acid are interesting, because vinegar, which is sometimes largely consumed by a certain class of individual, contains 4 per cent. glacial acetic acid. I found a 0.5 per cent. solution, or one of any strength above this, to markedly increase the lesions produced by gastrotoxin.

Lactic acid, on the other hand, which is formed by bacterial fermentation in the stomach under certain conditions, is not so potent in this respect, but 3 or 4 per cent. solutions will cause an increase of the lesions.

The rapidity of formation of an ulcer of the stomach varies according to the activity of the gastric juice. After a meal of 100 to 120 grams of meat the cat's stomach is usually not empty till a period of about twelve hours has elapsed, whilst after a meal of 6 oz. of milk it is empty in three hours. The stomach of a 3,000-gram cat will hold, when fully distended, a little over half a pint, but the animal will only drink about 6 oz. at once.

I conducted a series of experiments in which half the animals were fed on meat and half on milk, an ulcer being produced in each cat by local injection of immune goat's serum. When the animal is fed on a meat diet, so that the walls of the stomach are exposed to the prolonged action of the gastric juice, the slough is rapidly formed, and has separated by the fourth day in most cases, a clean ulcer resulting. In the case of animals fed on milk, however, the period of formation of the ulcer is longer. In two cases the slough had not separated on the seventh day, and in two other animals, killed on the eighth and eleventh days respectively, a small slough was still adherent to the centre of the ulcer. The same result is seen in the case of fasting cats. When the stomach is empty the ulcer may not appear at all, or, if it does, it is quite superficial. We see therefore that, when all the conditions necessary for the production of ulcer are present, the rate of formation of such an ulcer and also its extent are largely dependent upon the condition of the gastric juice. In the human being acute ulcer is initiated in several ways, but in all cases, by the action of the gastric juice, the different lesions are eventually converted into precisely the same kind of ulcer, so that in most cases it is impossible to say how the ulcer took its origin.

I now pass to a consideration of the influence of the gastric juice upon the healing of experimental ulcers.

The healing of acute ulcer was first studied in 40 guinea-pigs and 21 cats on a normal diet, and I found that in all cases healing occurred in about three weeks, the exact time depending on the size of the ulcer.

In order to test the effect of hyperacidity of the gastric juice, ulcers were produced in 24 guinea-pigs. Half the animals were put on a normal diet and half on an acid diet, which consisted of food soaked in 0.6 per cent. HCl. I had previously found that by feeding the animals on such a diet it was possible to keep the stomach contents permanently hyperacid to the extent of about 0.27, or 0.3 per cent. This investigation showed that the ulcers in the animals fed on an acid diet passed through precisely the same stages as those of the animals fed on a normal diet, and that they healed up in the same time. The same results were obtained in a series of cases in which a diminution of the acidity of the stomach contents was present. Half the animals were fed on food soaked in a 4 per cent. solution of sodium bicarbonate, which effects this result, and half were fed on a normal diet. In each case healing occurred in practically the same period of time.

When the motor power of the stomach is interfered with, however, a definite delay in the healing of the ulcer occurs. Pyloric stenosis was produced in cats by constricting the first part of the duodenum with a piece of rubber tubing.

Retention of food in the stomach was thus produced and consequent dilatation of the organ. Ulcers were produced in a series of cats having pyloric stenosis, and in normal cats as controls. I found that motor insufficiency of the stomach definitely delayed the healing of the ulcers for at least twice the normal time.

The delay in the healing occurred during the early stages, before the single layer of cells, which eventually develops into glands, had completely covered the base. The cause of this delay was not due to a fault in the epithelium but to necrosis of the connective tissue base of the ulcer—so that the epithelial cells had no granulation tissue over which to grow—and also to excessive formation of fibrous tissue in the base, the young glands having no cellular stroma in which to proliferate. These conditions were due to the retention of food in the stomach, which allowed of a prolonged action of the gastric juice upon the connective tissue base of the ulcer.

The time of healing of acute ulcer also depends upon the quality of the food, for the same reason. We have seen that meat is retained in the stomach longer than milk, and that it produces a greater flow of gastric juice. In a series of experiments in which half the animals were fed on meat and half on milk, I found that the ulcers of those fed on milk healed up more rapidly than those of the meat-fed animals.

The delay occurred in the early stages of healing and was due to the condition of the base of the ulcer. When once the base was covered with a single layer of epithelial cells so that it was thus protected from the action of the gastric juice, the further changes in this layer progressed at a rapid rate.

The glands of this regenerated mucous membrane were more irregular and not so perfectly formed as in the condition of normal healing, because of the excessive formation of fibrous tissue in the base, resulting from the irritation of the gastric juice.

In the treatment of a case of gastric ulcer, therefore, the acidity of the stomach contents should be kept low by the administration of alkalis. The diet should be free from irritants, and should consist of foodstuffs which remain only a short time in the stomach and excite only a moderate flow of gastric juice. If there is present dilatation of the stomach and retention of food owing to muscular insufficiency, which resists medical treatment, or pyloric stenosis, gastro-enterostomy should be performed.

DISCUSSION.

Sir BERTRAND DAWSON said: No one worker has contributed so much to our knowledge of gastric ulcer as Dr. Bolton. The present paper is, if possible, even more interesting than his previous contributions, because it throws some light on the formation of chronic ulcer, and it is the chronicity of ulcer in the human being which needs explanation. Clinical experience bears out the view that the stage of acute or mucous ulcer often shows evidence of microbic or toxic invasion of the stomach, whereas in the stage of chronic ulcer the chief factor at work is the hydrochloric acid. Stomachs likely to be the seat of mucous ulcers usually show normal or lowered hydrochloric acid content, and their fasting contents disclose microbic growth, whereas most chronic ulcers have a raised hydrochloric acid content, and their fasting contents are sterile. The observation that other acids will favour ulceration is important, and the speaker inquired if Dr. Bolton had made any experiments with citric acid. The reason why the cats who were fed on meat healed more slowly than those fed on milk was probably because the stomachs containing meat emptied more slowly than those containing milk. Middle-aged people with gastric ulcers show evidences of delayed stomach emptying independently of pyloric constriction.

SECTION OF BACTERIOLOGY.

Professor JAMES RITCHIE, President.

DISCUSSION ON VACCINE-THERAPY.

OPENING PAPERS.

I.—JOHN EYRE, M.D., M.S.Durh.,
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PREPARATION AND STANDARDIZATION OF VACCINE.

SPEAKING of prophylactic inoculation, some ten years ago it is true, Wright said that the best results would not be obtained until perfect asepticity and *accurate standardization* of the vaccine had been secured. But this dictum applies just as forcibly to therapeutic inoculation, and yet at the present day it would be difficult to find half a dozen men who employ the same technique in the preparation and standardization of their vaccines.

I therefore propose rapidly to review the materials and methods available in this connexion. I cannot, however, refrain from touching first upon that much debated point—the relative values of stock and autogenous vaccines.

There is much to be said in favour of each. Speaking generally, it will be conceded that the natural progress of any bacterial infection is in the direction of recovery, and that the natural process of recovery consists in the elaboration of a sufficiency of specific antibodies in response to the stimulus afforded by the infecting bacterium, to ensure the neutralization of its noxious products, and eventually the destruction of the infective agent. Given accurate methods of bacterioscopic diagnosis, intelligently and carefully applied, so that the isolation of the infective agent is attained, we have in the autogenous vaccine subsequently prepared a material which will provoke the formation of antibodies absolutely specific for the bacterium responsible for the infection.

Now, it is a matter of common knowledge that if members of the same species of bacteria are isolated from different sources, the biological characters exhibit such a close family resemblance that we are justified in applying the same type-name to each; yet, when we study each strain minutely, it is quite usual to find that one strain possesses some peculiar feature which is not shared by the rest. Some years ago Washbourn and I isolated four strains of the pneumococcus from four different individuals, and, after some months of study, decided that they were absolutely identical in all their characters. We then tested the protective properties of an antipneumococcic serum prepared by Pane against each of these strains in turn, and discovered that though efficient in the case of each of three of these strains, the serum was powerless to prevent the onset of fatal septicaemia in those animals inoculated with the fourth strain. In other words, the antibodies elaborated in response to the stimulus provided by some strains of pneumococci are not sufficiently specific to ensure the destruction of all strains of pneumococci.

Again, it is common knowledge that certain biological characters of bacteria can be profoundly influenced by variations in the environment. Penfold has shown that by repeated passages *in vitro* through media containing lactose, a strain of the typhoid bacillus can become endowed with lactose-fermenting powers. Analogous changes can be induced in bacteria *in vivo*; thus, by carefully arranged passages, a strain of *M. melitensis*, originally almost without pathogenic properties for the rat, when inoculated in any way other than intracerebrally, can be taught to produce fatal results when injected subcutaneously into that rodent. Again, I have observed during the course of a *B. coli* cystitis treated with vaccine that the infecting organism undergoes minor changes in its biological characters, which necessitate the preparation of a fresh vaccine from this modified bacillus, if a complete cure is desired. The final suggestion I would put before you concerns the infection where it is impossible to identify the responsible organism—although it has been obtained in pure culture—without an extended series of cultural and other observations. Here identification can wait until the vaccine has been prepared and the treatment of the patient initiated. All these points emphasize the necessity for the employment of autogenous vaccines whenever possible.

But an autogenous vaccine is not always possible, and under these circumstances stock vaccines find their chief application. Time in treatment is a very important factor, and the preparation of, for instance, a vaccine of the tubercle bacillus occupies weeks, or even months; a streptococcus from the cellulitis affecting the begrimed hand of a working man out-patient may require several days for its isolation in a state of purity; it may not be possible to isolate the gonococcus from a female arthritis case until the next menstrual period—perhaps two weeks distant. In all these instances it is preferable to avoid delay by instituting treatment with a stock vaccine, since stock vaccines can always be kept on hand ready for use. The supply, too, can be replenished from individual organisms as they occur in the routine work of the laboratory—the stress and strain of isolating bacteria in a minimum number of hours is thus avoided. But stock vaccines have two disadvantages—both real. The first and most important I have already mentioned—namely, that the antibodies elaborated in response to the injection of stock vaccine may not be sufficiently specific to effect the complete cure of the patient. The second concerns the use of vaccines by those who have had but little experience in bacteriological work. The man who has had one or two successes in the field of vaccine-therapy is sorely tempted to treat subsequent bacterial infections with stock vaccines of what he *thinks* may be the responsible micro-organisms, without the essential preliminary of a bacteriological diagnosis. In the early days of diphtheria antitoxin we know many cases of pneumococcic or pneumobacillary membranous sore throat were treated with serum, and the absence of beneficial effects led to adverse criticism of serumtherapy by isolated individuals, and a subsequent reluctance to exhibit it in cases of true diphtheria. So in the present day a man might be induced to administer stock vaccines of streptococcus or catarrhalis in a case of arthritis associated with a pyorrhoea due in reality to *Staphylococcus pyogenes aureus* infection. No harm would be done, it is true, beyond a disappointment to the patient and the loss of valuable time. Next with regard to the question of virulence. Here I would express my conviction that a virulent culture provides a much more satisfactory vaccine than one prepared from an avirulent strain of the same organism, although I am fully aware that in prophylactic inoculations—as, for example, against typhoid—it does not appear to detract from the value of the vaccine when it is prepared from attenuated laboratory cultures far removed from the human body. This fact in no way militates against the conviction I have expressed. A vaccine made even from an avirulent culture may provoke the formation of sufficiently specific antibodies to ensure the destruction of a fairly large dose of virulent bacilli when first introduced into the body, whilst similar antibodies formed in response to the same stimulus might well be insufficiently specific to neutralize the noxious matters elaborated by those virulent bacilli after they had established a footing in the tissues of the host. From what I have already said, it will be obvious that if I am forced to use a stock vaccine, my preference is for one compounded from many different strains, rather than from a single representative of the required species, since when a large number of strains are employed there is a greater probability that one or other may provoke the formation of the antibody to the peculiar toxin of the organism which is responsible for the infection to be treated.

Turning now to the preparation of a vaccine, my remarks bear primarily on the preparation of an autogenous vaccine; at the same time but little variation will be needed to fit them to the preparation of stock vaccines. First, rapidity is an essential factor in the isolation of the responsible microbe. The fewer subcultivations that are needed the more closely will the organism, when eventually isolated, correspond to the one residing in the patient's tissues. Perhaps as a counsel of perfection, to which, however, I personally endeavour to attain, I would say that no organism further removed from the human body than two subcultivations upon nutrient media shall be used in the preparation of vaccine. In so brief a period as this, practically never extended beyond forty-eight hours, the amount of attenuation that the organism undergoes is a negligible quantity, whilst its fermentation properties, as tested by the various sugar reactions, will have undergone no material change. This rapidity of isolation predicates something more than the shrewd suspicion as to the identity of the organism under observation, it demands an intimate acquaintance with the biological characters of that organism. Many are the devices that on occasion have to be resorted to in isolating the responsible microbe. Sometimes we depend upon selective media in the preparation of our cultivation; for example, *Bacillus coli* may be readily isolated if the material is at first inoculated into bile salt broth, incubated at 37° C. for about four hours, and then plated upon nutrose agar. The pneumococcus, on the other hand, is very often more conveniently isolated by inoculating the material directly into a rabbit and recovering the organism in pure culture from the heart blood.

Again, antiserums of high agglutination titre are often exceedingly useful. I have recently had to isolate *M. melitensis* from an impure first culture in which the associated organism was *Bacillus subtilis*. Here the application of *melitensis* antiserum completely agglutinated the cocci, and a pure culture was readily obtained from the deposit. On another occasion, a mixture of *Streptococcus longus* with *Bacillus coli* prompted the converse of

this method of procedure. Thus *coli* antiserum was added to the mixture and the streptococcus obtained in pure culture from the supernatant fluid after centrifugalization. The actual culture to be employed in the preparation of the vaccine should be one grown on a solid medium under optimum conditions, that is—optimum reaction of the medium, the optimum temperature, the optimum atmospheric conditions, and, above all, for that incubation period which will give the maximum of living virile bacteria and the minimum of dead and dying individuals. Suspension of the bacterial growth should be carried out in a fluid menstruum of sufficient density to obviate either plasmolysis or osmosis. The fluid I prefer is 0.1 per cent. solution of sodium chloride. The bacterial growth may be first washed from the surface of the medium by spraying with this fluid, or it may be removed from the surface of the medium by the aid of a platinum spatula, and mixed with the fluid.

Whichever plan is employed, a homogeneous emulsion must be ensured by thorough agitation of the mixture. I think all will be agreed that for this purpose an electrical shaker has no equal, while the addition to the mixture of sterilized glass beads promotes an even emulsion. Occasionally it happens with members of the *catarrhalis* group, or of the diphtheroid group, that clumps of bacteria resist all attempts at separation. Suspensions containing such clumps should be thoroughly centrifugalized, and the deposit discarded. Attempts at standardization of the resulting homogeneous emulsion have been practised by bacteriologists since vaccines were first employed. In a paper published more than eighteen years ago, the directions for preparing an anticholera vaccine were practically these:

> Use glass test tubes 16 cm. long by 1 cm. diameter, containing 10 c.cm. of sterilized agar. Slope the agar in the tube at such an angle that the surface of the medium is 10 cm. long, inoculate with the cholera vibrio, incubate at 35° C. for twenty-four hours. Emulsify the growth in 6 c.cm. of half per cent. carbolic acid solution, and when the germicide has killed the bacteria in the emulsion, divide the emulsion into six doses.

The many fallacies involved in such a method of standardization are so obvious that I need hardly refer to them. Another ingenious method of standardizing was one used for a time by Wright, in which the opacity of the emulsion formed the basis of standardization, and, when necessary, the emulsions were diluted until a layer 1 cm. in depth was just sufficient to obscure the image of the test object when viewed through it. Gravimetric methods of standardization have frequently been proposed, and will ultimately, I believe, supersede all others. They are, however, all subject to the serious disadvantage of requiring considerable time for their execution. As we shall shortly have before us a communication dealing with gravimetric methods, I do not propose to say more about them here.

The most convenient methods of standardization in use at the present time deal with the actual enumeration of the micro-organisms in the emulsion. This may be done directly with the help of a haemocytometer chamber—a method which gives in practice very accurate results. The tiro, however, will find that the active Brownian movement interferes to a certain extent with the accuracy of his results, and many of us use intra-vitam stains, such as neutral red, for the purpose of simplifying the method. Enumeration by plating has many advocates, but here again the time factor militates against its general adoption. Two, and sometimes three, days' incubation are essential, if all the individual micro-organisms deposited in the plate are to have time to form visible colonies; and the method of decimal tube culture has to a large extent superseded plating. In this, as the name implies, the emulsion is diluted, and decreasing tenths of a given unit are added to successive tubes of fluid media. The majority of us, however, enumerate by means of stained films in which comparable volumes of the emulsion, and of standardized "known" emulsion, are mixed and examined microscopically; or preferably equal volumes of the emulsion and normal human blood.

In either of these methods a perfectly homogeneous emulsion free from clumps is essential for accuracy. With a standardized emulsion as the known quantity errors are frequent, even when formalin has been used for killing the organisms in the emulsion, owing to variations as time goes on in the staining reactions of the organisms, so that, after all, the use of human blood for comparison is probably as accurate as any method, and given normally healthy individuals working in a laboratory, the variation in the red corpuscle content of the unit volume in different workers is a negligible quantity. The sterilization of the emulsion after standardization is carried out by many by a process of autolysis. Others sterilize by the addition of comparatively powerful germicides. With these two methods I do not propose to deal, as, owing to the inherent biological qualities observed even in different strains of the same species of bacteria, sterilization may not be completed for a considerable period.

> For example, out of eight strains of typhoid bacilli which were permitted to autolyze, three were sterile in twenty-four hours, another three in forty-eight hours, the seventh strain at the end of six days, and the eighth not until ten days had elapsed. Of the same strains sterilized by the addition of a half per cent. phenol, six were killed within twenty-four hours, the remainder not until the end of four days.

Mechanical disintegration in some form of grinding apparatus preferably operated at the temperature of liquid air is probably the most satisfactory method that can be used. For not only is it efficient and rapid, but, as we know from the observations of the late Dr. McFadyean, it sets free antigens which are not available in the vaccine as ordinarily prepared. Unfortunately, the cost of the necessary apparatus is too great to permit of its general application. So that, after all, sterilization by heat is the method that most of us employ.

In this connexion I would suggest that the lowest possible temperature which suffices to kill the individual bacteria is the most suitable for our purpose, since it is essential that changes of the molecular arrangement of the bacterial protoplasm, particularly in the direction of heat-coagulation, should be carefully avoided. I would further suggest that exposure to the thermal death point of the bacterium employed for a period of one hour is the most suitable. The sterility of the emulsion should be determined subsequently; but experience will show that if attention is paid to the maintenance of the requisite temperature throughout the exposure—by the adoption of some method for keeping the water in the water bath in constant movement—sterilization is effective, and the vaccine can be used with confidence, even before its sterility has been confirmed by the lack of growth in the subcultures.

The final stages in the preparation of the vaccine—such as the dilution, the bulbing or bottling, etc.—I do not propose to deal with.

With regard to dosage, I would only express the opinion, founded upon my own experience, that when virulent autogenous vaccines are employed, care should be exercised in the selection of the initial dose. This should be small, probably not exceeding 5 million, whilst, when dealing with attenuated stock cultures, the initial dose may be anything from ten or a hundred times as large. Again, it has been stated that age does not affect the immunizing qualities of a vaccine. My own impression is that, as a vaccine grows old, the dose must be very considerably increased to obtain similar results, and so strong is my conviction upon this point that at the end of six months, if needed for so long a time, I destroy a vaccine and prepare a fresh brew.

To summarize: my preference is for a virulent autogenous vaccine not more than two generations removed from the human body, cultivated under optimum conditions, suspended in 0.1 per cent. saline, emulsified by shaking with glass beads in an electrical machine, centrifugalized if necessary, standardized against normal blood, sterilized by exposure in a water bath of a temperature corresponding to the thermal death-point of the organism in question for one hour, dilution to the requisite extent with 0.25 per cent. tricresol solution, and employed in small doses during its efficient period of from three to six months. From what I have already said it is obvious that there are many points upon which vaccine-therapists might agree to adopt uniform methods, but such is our ignorance—or, speaking only for myself, my appalling ignorance—of many fundamental facts in bacterial biology, that I fear the time is not yet ripe for absolute uniformity of procedure, even in the preparation of vaccines.

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LABORATORY TESTS AS A GUIDE TO INOCULATION TREATMENT.

MOST of us in this Section must consider that vaccine-therapy is now an important method of treatment, and every one must realize its great possibility for the future. The only question is how these possibilities can best be realized. At present there seems to be a tendency—quite a natural and easily explainable tendency—to underestimate the help which may be derived in the future from laboratory testings. In considering the help derived, and to be derived, from laboratory work it is necessary to go back to the beginning. How did vaccine-therapy as we know it arise? We all know that what we now call vaccine when inoculated into an experimental animal will produce immunity against the substances in those vaccines, and this immunity can be demonstrated in samples of blood taken from the animal. If the vaccines are of dead bacteria, then by laboratory tests we can show the production of immunity against living bacteria of the same species. Without this demonstration it is difficult to imagine how vaccine-therapy could have been developed. People might have advocated it as "toxi-therapeutics" on the principle of "the hair of the dog that bit you," but they would have obtained no following.

Without laboratory demonstrations of changes in immunity the idea of vaccine-therapy is irrational; with them it is not only rational but obvious. These laboratory tests are, then, the very foundation stones on which vaccine-therapy is built.

So much for these laboratory tests being the foundation stones of vaccine-therapy; let us see how inoculation treatment was built on this foundation. It was found that when an infected animal was inoculated with a dead vaccine corresponding to the infection changes were produced in immunity, and suitable doses would produce an increase in immunity. These changes were demonstrated by laboratory tests of the animal's blood, and in this way and by these tests a system of dosage was built up. Now, proof of the correctness of this building up is afforded when, on looking back at a long series of cases, it is obvious that they have been deriving benefit from the vaccines.

Now it may be said that this benefit to the patient has been—or, at any rate, might have been—the guide; but I think that any one who has tried to build up a system of doses under such a guidance will know how difficult it is; for in giving a dose one is not inoculating a long series of cases, one is inoculating an individual man, woman, or child, and the effect of the dose is masked by extraneous circumstances; one is left wondering whether the patient got well because of, or in spite of, the dose. May I give two examples of this difficulty? During the last three years I have been treating children suffering from whooping-cough with a vaccine made of Bordet's bacillus; unfortunately, I can make no blood test as to the efficacy of each dose, and it is quite impossible to judge from an individual case whether the doses are doing good or not. I therefore made statistics with a most elaborate system of control cases, and these statistics point to good having been done by the vaccine. But I have an uncomfortable feeling that statistics can be made to prove anything, and I hesitate to advocate this treatment; but, given a moderately reliable blood test, and I feel I could clear up my doubts in a couple of months. The other illustration is that of vaccination for hay fever. Time and again active immunization has been attempted—from the homœopaths who made one smell hay during the winter, down to Dunbar's most careful and ingenious work—but always without success as far as active immunity was concerned. Then my friend Mr. Noon demonstrated with a quantitative ophthalmo-reaction that he could produce an increase of immunity by inoculating suitable doses of a pollen extract. The results of treatment built up on these lines have emphatically proved the value of his laboratory testings.

Have, then, all inoculations been controlled by tests? We know that they have not. Owing to the great difficulties of the tests it is often unreasonable or even impossible to make them. But in a way it may be said that all doses have been controlled to some extent by blood testings. Information obtained by laboratory work on one occasion will be of help on another occasion—perhaps with a different patient, perhaps to a different inoculator, perhaps it will even be used by analogy in combating a different micro-organism; those inoculators who say that they do not believe in laboratory tests are in the last resort acting under the guidance of those who do. Even those in this country who in the administration of tuberculin make it a boast that they reject all help which did not come from Koch are building over a foundation of blood tests. When Koch commenced his inoculations there was loose talk of "toxi-therapeutics," but as knowledge accumulated so the idea of immunity became more definite. The demonstration that a dose of typhoid vaccine would increase the typhoid agglutinating power of the inoculated animal suggested to Koch that his doses of tuberculin might be controlled by measuring the tubercle-agglutinating power of his patients. He well knew the value of specific laboratory guidance in supplementing the non-specific clinical evidence, although the agglutination of tubercle bacilli was and is a tricky and uncertain procedure. He said:

> The attempts to protect people against tuberculosis by immunization have been tried for several years, but they have been greatly handicapped because we possessed no reliable means of telling what result had been obtained with each single attempt. But the former uncertainty is at one blow removed now that the agglutination reaction puts in our hands a means of making certain, step by step, whether we in our attempts are working in the right direction.

When Koch visited the inoculation department of St. Mary's Hospital shortly before his death he was considering the possibility of obtaining guidance from the complement-deviation method. He was keenly interested in the use to which we had put the tuberculo-opsonic index in following out the effect of doses and studying autoinoculations. He expressed the opinion that future advances in the use of tuberculin could only come about by similar careful study of the changes produced in the blood.

So much for the help we have derived from blood tests in the past. Will they be required in the future? We can do without them only if all is now plain sailing. But we know that our paths are simply beset with problems. Take, for example, the case of an infection where the lesion is extensive and hidden; we know that Nature's method of cure is for the microbic substances of the lesion to stimulate the body to the production of immunity—by autoinoculation, in fact. The giving or withholding of a vaccine should entirely depend on whether these autoinoculations are insufficient, adequate, or excessive.

How can one tell of this? Surely the most satisfactory way is by blood testing. I am tempted to say that the only way beyond guessing is by blood testing. Then again, if a dose of the appropriate vaccine is given, in such a case its effect will be at least twofold: it will initiate some process of immunity on its own account; it will also provoke an autoinoculation from the lesion which will stimulate a second process of immunization; and this, again, may react on the lesion, and so on. I take it that the different doses given—say, of tuberculin—in cases of phthisis are different ways of meeting these difficulties; you may perhaps give so minimal a dose as to produce some small quantity of immunity and yet no autoinoculation; a larger dose may produce an autoinoculation of just a satisfactory strength for the patient; or one may attempt to produce such a degree of immunity with the dose that the body can snap its fingers at any attempt of the lesion to autoinoculate. The results may be good or harmful, and this will depend on whether the dose is appropriate to the extent of the lesion and its autoinoculation.

Clearly here are difficulties, and clearly here we need help. How shall we pick our way? How shall we even tell whether a method has done good or harm? Hardly by the help of statistics, for the cases would not be comparable; in all probability a man following one method would pick suitable cases, while another man would take the derelicts; and, in any case, "results" depend more on the sanguine temperament of the recorder than on anything else. On the other hand, a phenomenon of immunity observed in the blood as the effect of a dose will give us specific information; this may be understood or misunderstood, but if it is in the least degree understood it will help us with other cases.

The autoinoculation difficulty is not the only problem awaiting solution: any one who has attempted vaccine therapy could give a string of them. For instance, what effect on immunity has rest, or fatigue, or food, or fresh air, or worry? What is a good constitution? What is "feeling fit"? One need not labour the point, for there are obviously plenty of problems. If we can get reliable specific information as to what changes in immunity are going on in a patient, we shall be enormously helped in treating that patient with a vaccine.

Just one more glance at this side of the question before we consider the disadvantages of laboratory testing. Sometimes the connexion between an inoculation and the foundations of vaccine-therapy become too flimsy to be safe. Nearly every one here must have come across examples of this in the case of doctors habitually using vaccines who do not know the difference between a vaccine and a serum. This suggests to me a man who talks about cricket and who yet believes that they use goal posts in the test matches. If such a man had ever had his attention directed to laboratory testings he could never have gone so far astray. If we once lose sight of the guidance of immunity, it seems to me that we shall take to giving vaccines like so much tonic.

Let us now consider the *disadvantages* of laboratory testing—and some of these are very real, some are exaggerated, and some, I think, untrue.

In the first place there are the difficulties of carrying out laboratory tests—physical, technical, financial, and so on. Probably time will produce new methods or will lessen the difficulties; but doubtless for this reason alone nine-tenths or even ninety-nine hundredths of inoculations are and will be given without testing.

So much may be granted; but it may be further urged that laboratory methods are not only difficult but fallacious. In the first place it has been said that the functions of shed blood have nothing to do with the powers of immunity of the patient. Take, for example, the agglutination test in typhoid fever; it is true that Widal proclaimed that the reaction now known by his name has nothing to do with immunity, and that he would renounce all claims to credit if this were so, but no one believes this now, and his statement has been forgotten or forgiven. The bactericidal powers of the blood are clearly protective in function. That extraordinary phenomenon phagocytosis cannot be an accident; every one must believe that it is used by the body to guard against bacteria; and if so, then anything which affects phagocytosis, by which I include the opsonic power of the blood, will clearly affect our powers of immunity.

All this will probably be granted; it is more likely that critics will say that the laboratory methods of testing these things are inaccurate, and therefore misleading.

In the first place I should point out that if vaccine-therapy has been built up on and by blood testing, then the success of vaccine-therapy is itself a proof of the soundness of the blood testings. But setting this aside, let us consider the reliability of the methods. Of course, it is not possible within the space of a few minutes to consider the accuracy of all laboratory methods; let us confine our attention to the one most often employed—that is, that of the opsonic index. It is most often employed because, as opposed to most others, this method is of general application, and also seems to register changes in immunity more quickly than other methods will do. When in addition to its more frequent use one considers that it is also the most difficult and troublesome, it is easy to understand that it is likely to be the method most often criticized. I have even heard it suggested (generally on the telephone) that the opsonic index, or the opsonic theory (whatever that may be) is exploded. What about this?

I take it that no one doubts the opsonic function of the serum; that can be demonstrated to any one capable of receiving a demonstration, and is just about as certain as the clotting power of the blood. But people might possibly doubt whether there is a difference in this opsonic function in health and disease, and, if so, whether the difference can be measured. Let me give a case which I think proves that it can.

My colleague, J. Wells, died of a laboratory infection; he felt a chill and found he had a raised temperature. There were no physical signs whatever. His blood was tested to every organism which seemed likely—typhoid, para-typhoids, tubercle, glanders, Malta fever, and so on. To all he was at first normal, but later, while remaining normal to the others, he was found one day to have an opsonic index of 1.4 to glanders, which on the next day rose to 1.87. From that time forward the patient's glanders opsonic index fluctuated, and was generally between twice and three times normal. For weeks and weeks no other help to a diagnosis could be obtained, till at length there developed in the calf of his leg an abscess, which yielded glanders bacilli.

This is more than an instance; it is wellnigh a crucial experiment, and such examples could be multiplied almost indefinitely to show that this opsonic test really does correspond to some change in the immunity of the patient. And if this is proved, then we must take it that the big fluctuations shown by the opsonic index to occur owing to autoinoculation are due to some real change in immunity; and if the changes after autoinoculation are real, then surely the changes detected after inoculation are real also. And if the big fluctuations in the index correspond to changes in immunity, why not the small ones? Clearly the whole question is one of degree of accuracy; as one looks for smaller and smaller differences, so the errors of technique will play a larger and larger part. If they are small they should not be disregarded, but taken with a grain of salt.

What of the accuracy of the tuberculo-opsonic index. It is in dealing with the tubercle bacillus that the method is most often employed, and it is here that one finds most criticism. Now, in the first place, one cannot have a glanders opsonic index which means something and a tuberculo-opsonic index of no value, but in any case it is easy to give instances where the tuberculo-opsonic index is obviously giving useful information about immunity in the patient. Perhaps I may quote one. The treatment of a phthisis case was to be controlled by blood tests; the doctor reported that the case was an extremely early one, with practically no rise of temperature, and a very small and apparently stationary lesion; in fact, he described it as an eminently suitable case—bound to do well. Blood tests showed fluctuations in the opsonic power indicating that big autoinoculations were going on. In a series of letters I urged the doctor to keep the patient more rigidly at rest, but I could not stop the autoinoculations, and therefore did not commence inoculations, to the annoyance of the doctor, from whom I heard no more. Four weeks later I heard that the patient was dead; I do not think with our present knowledge we could have saved him, but certainly if a dose had been given his rapid collapse would have been ascribed to it.

Here, again, the question is, How small are the changes which can be measured without being unduly masked by errors in the technique? Personally I am convinced that the method can be used with very great accuracy, though, of course, it can be very inaccurate on occasion. This inaccuracy would be a danger, but we have a great safeguard in the fact that the man who does the work always has a very good idea as to its accuracy; this is particularly the case if he is of a critical temperament, and controls his work effectually by testing several normal bloods. If the worker himself is allowed to interpret the meaning of any figures he obtains, he will know how big a grain of salt to apply to them.

It is, of course, not the index which is of value, but the interpretation placed on it as indicating some change of immunity in the patient; in this connexion it may be pointed out that more information can be obtained (and the method made safer) if a series of blood specimens are tested together, and attention is paid not only to individual indices, but also to the trend of any changes in those indices. This question of interpretation brings me to my last point with regard to the opsonic index—the results may be perfectly accurate, but their meaning misunderstood (may even be unexplainable at present). One remembers how physicians denounced the Widal test as faulty merely because patients with typhoid fever (but making a poor fight against it) failed to form agglutinins. The same sort of thing happens with the opsonic index, owing to insufficient knowledge. For example, a senior physician several years ago wished to "test the value of the tuberculo-opsonic index." For this purpose he admitted a patient with phthisis, and the blood was found to have

an opsonic power of 1.45 to tubercle. After a month of careful nursing and rest in bed the index dropped to slightly subnormal on several testings—which, of course, is the result to be expected. But the physician's comment was "I take him in ill with a high index, and then send him out cured with a low index; what is the good of the index?"

The alternative to blood testing is clinical evidence as a guide to inoculation, and of course no sensible man will neglect any clinical evidence he can get, and this will be of special value where the lesion can readily be observed. But the absence of any specificity of clinical evidence is a terrible handicap; anything which happens to the patient after the dose tends to get credited to the dose although it may be a sheer accident, and one has to pick out the significant facts from a mass of irrelevant details. Even the temperature of the patient, which is supposed to have a direct bearing on immunity, may remain normal while quite considerable immunization processes are proceeding; and if the temperature is abnormal it is quite non-specific, and so may be misleading.

I should like to quote cases to show how misleading a combination of extraneous circumstances and the imagination of the patient may be. Numbers of them occur to me, but I must cut myself down to one. In the Inoculation Department of St. Mary's Hospital it is often desirable to make a serum diagnosis of the infection before proceeding to treatment. It saves explanations if the patient is allowed to think that a small quantity of salt solution inoculated subcutaneously is the first dose of vaccine. At the next visit I record the result of this supposed dose. About 20 per cent. say that it had no definite effect, and 40 per cent. that it did good, and 40 per cent. that it did harm. This is, of course, because anything which occurred to them after the dose tends to get ascribed to it. Occasionally the patient gives surprisingly precise details. For example, one patient told me that she had felt "all overish" on the bus going home, and thought she was going to faint; she felt drowsy that evening and next morning, but after that began to improve, and since then had been feeling better than she had done for years. All this produced by ¼ c.cm. of salt solution. Now one does not want vaccine-therapy to be built up on non-specific evidence of this sort.

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ON THE METHODS FOR STANDARDIZATION OF BACTERIAL PREPARATIONS FOR INOCULATION.

THE desirability of a common method of preparation and standardization of such potent agents as bacteria used for inoculation is obvious. Both the qualitative and the quantitative factors demand consideration, but at present in regard to neither is there any uniformity of agreement as to the methods which are most suitable.

With regard to the qualitative standardization, which is the more difficult problem, we lack still any numerical basis for estimating the potency of a suspension of dead bacteria. The estimation of toxic power, or minimum lethal dose, is not suitable, working as we are with dead bacterial bodies freed from exotoxins. As for toxin action due to the presence of endotoxins, this does not furnish a practicable subject for standardization, and it is open to question the degree to which the immunizing reaction which is aimed at in inoculation is bound up with the production of anti-endotoxins. The apparent toxic effect which the inoculation of bacteria may have on an individual suffering from disease is widely different from any effect produced in a healthy individual.

The most reliable indication as to the potency of a bacterial preparation would be afforded by the degree of immunizing response set up by its injection into a healthy animal. Here it would be necessary to decide which aspects of the process of immunization demand consideration. But if arbitrarily we take only the phagocytic power of the blood into account, we are faced with another factor over which we have no control—that is, the variations manifest between different individuals in the way in which they respond to an inoculation. Still, there is the possibility that it might be found practical by averaging to determine a "minimum immunizing dose" for any preparation—that is, the smallest average dose which serves to produce or to increase demonstrably the immune substances present in the blood.

At the present time the general agreement seems to be that the most that can be done is to aim at the highest degree of efficiency attainable, which implies the greatest potential pathogenicity. In the absence of any physiological standard obtained by experiment, it is probable that this aim at high potency is quite a satisfactory means of qualitative standardization. It corresponds very much to the aim at securing great chemical purity of alkaloid substances, apart from physiological tests. The method to be employed for this purpose is to obtain a pure culture from a highly virulent infection, without recourse to subculture; to employ that culture medium which gives the most active growth and which best conserves its pathogenicity, and to make it up at that stage of the growth when it contains the greatest number of active individual organisms before the stage of degeneration, involution, and death has developed to any extent. In its sterilization the least possible disorganization of the bacterial protoplasm is desirable. These various factors differ, of course, with individual types of bacteria or individual strains. The microscope, I think, is a useful aid toward deciding the best stage of growth at which to make up a preparation and the comparative value of different modes of sterilization, since the staining capacity of the organism supplies a fair measure of the preservation of the bacterial protoplasm.

Though a qualitative standard is still lacking, there is no reason for neglecting the importance of a quantitative standardization any more than we do with pharmacopœial drugs not physiologically tested. Although the standardization of New Tuberculin preparations by a quite satisfactory method of weighing has been before us as an example, the method of enumerating bacteria has been commonly adopted and has only tended to confusion in dosage. The ingenious method of Wright has been generally made use of, perhaps on account of its being considered a simpler and shorter procedure. Actual experience does not bear this out. With certain bacteria which tend to auto-agglutination, the securing of a suspension as uniform as is necessary is tedious or unsatisfactory. The counting of the blood corpuscles for the test adds to the time taken up. Error may arise from the staining of the bacteria being poor. The personal factor in the enumeration may furnish another. Then, in the case of coccal or diplococcal forms, it is an arbitrary matter what is to be counted as an individual, and the depth of staining may introduce difficulties in this respect. Again, the standardization is based on the enumeration of only a small sample of the whole. But the chief objection to enumeration is one of principle. If it is the mass of bacterial protoplasm which we wish to judge of, we are faced by the fact that organisms vary enormously in size in any one culture at different stages of growth and also as between different strains. In a bacillary culture, at any one time, there may be all grades between almost coccal forms and long filaments. Certain organisms, such as those habitually growing in filamentous forms, do not lend themselves to enumeration. This method also precludes any attempt to compare the doses for inoculation of different kinds of bacteria. A valuable paper[1] by W. James Wilson and C. Dickson has recorded the margin of error in enumeration tests, and has shown how much greater it is than that given by gravimetric methods.

I have long been convinced that a method of weighing was greatly to be preferred, and I have accordingly adopted it consistently for the past six years, and have no reason to discard it. The method adopted has been similar to that used in making Koch's tuberculin.

Bacteria are grown in whatever is the most suitable medium, most commonly an inspissated serum. If a bouillon is used, it must be free from any phosphatic deposits. The growth is transferred to centrifuge tubes, being washed off a solid medium with 0.9 per cent. saline solution. It is then centrifuged in an electric machine. The supernatant medium is removed by pipetting, and replaced by fresh saline solution. The tubes are then shaken up and again centrifuged. Then the saline fluid is pipetted off and replaced by a smaller amount of distilled water. The suspension, now free from culture medium and saline, is again finally centrifuged, and then the deposit is transferred by means of a pipette to an agate mortar. The addition of a few drops of distilled water serves to clear away any deposit adhering to the tube or the pipette. The deposit,

consisting now solely of the bacterial protoplasm, is dried out on the mortar in an oven or desiccator. It forms a scale on the surface, which is then rubbed down into a fine powder with the agate pestle, or is scraped off as a scale preparation and transferred to a glass tube, in which it may be stored indefinitely till required for use. For this, the whole powder is weighed in a chemical balance, or a desired quantity is weighed out. This is then ground up on an agate mortar in a few drops of saline solution to form a uniform paste, which is finally made up to the volume desired to give the stock suspension of suitable strength.

So far I have not mentioned the method of sterilization adopted. It has been my practice to use chloroform and toluol for preserving suspensions during the process of making up, if this was not completed straight away, and also for the final dilutions. It was found that this often served to sterilize the preparation satisfactorily, and I consequently adopted it, where rapidity was not of importance, in preference in suitable cases to the use of heat. The chloroform and toluol is added to the growth in the centrifuge tubes when first filled from the culture tubes. It has been found that coccal forms are readily sterilized by this means, while the bacteria of the *Coli* group are more resistant. Heat sterilization is, of course, necessary for certain forms of bacteria. In this connexion I may mention that at any rate tubercle, staphylococci and streptococci do not appear to be impaired in their immunizing capacity by a temperature of 100° C., but the rule to be followed is to use the lowest temperature necessary.

Apart from the technical side of this question, I should like to dwell with some emphasis on the clinical significance of the adoption of a gravimetric standard. One has found that, as far as one may judge of it from the study of the phagocytic power of the blood and also of the clinical results in disease, inoculations of bacteria produce, weight for weight, whatever their variety, practically similar effects. This observation has simplified the subject of dosage enormously. Instead of having to consider the number of millions of bacteria of each type which one is to inoculate, one has a common standard for all, which helps one's judgement greatly. The outcome is that similar doses of, say, tubercle, staphylococci, coli, etc., are used in infections with these organisms. It is wholly the clinical condition—its acuteness or extent—which determines the suitable dose.

REFERENCE.
[1] *Journal of Hygiene*, vol. xii, No. 1, May 3rd, 1912.

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THE object of all who belong to the progressive school of medicine is to convert medicine from an art into a science, that is, to replace opinions by figures. In many branches, notably in the treatment of infective diseases and in diseases of the cardio-vascular system, measurements of various kinds have been systematically made for several years, and considerable progress has been made towards putting the matter on a scientific basis. In immunity some of the most successful work has been done in prophylactic inoculations against typhoid, in the experimental study of which the bactericidal and agglutinative power of the blood was systematically measured, and the results of the treatment have been strikingly successful. All the early work of Wright's school in curative treatment by vaccines was also checked by the study of the opsonic index, with results that we all know.

One point has become abundantly clear in all these scientific experiments, that is, that the phenomena observed, being vital processes, are variable, and consequently, to obtain any information of value, it is necessary to carry out a large series of observations in each case. In experiments on blood pressure, for instance, carried out on the human subject, it is found that the inoculation of any drug will raise the blood pressure for the time being. The effect is a psychical one, and a series of observations on the blood pressure is required before the actual effect of the drug can be estimated. In the measurement of immunity, the test of the greatest delicacy is no doubt the estimation of the opsonic index, and the observations of Dr. Freeman first established the variability of the opsonic index in cases of infection, and an auto-inoculation experiment can be employed as a crucial test for infection.

We cannot, however, have it both ways, and if the value of the auto-inoculation test is the variability of the opsonic index which is thereby demonstrable, it follows that a series of observations must be made before any results of value can be obtained. This, of course, adds enormously to the practical difficulties of carrying out such observations, and those who have the good fortune to work in a laboratory devoted to observations of this kind often fail to appreciate the difficulties which stand in the way of those who are unable to take part in a similar division of labour. I have reluctantly come to the conclusion that it is a counsel of perfection to insist on the control of inoculations by the use of the opsonic index. In addition to that, the high degree of technical skill required is a serious drawback to widespread use of the method, for it is undoubtedly quite possible to send specimens of the same serum for estimation to several different laboratories conducted by capable authorities, and to get different results from each of them. I do not wish for a moment to suggest that the method is not one of value. The fact that in one laboratory alone four apparently healthy persons at work there have been diagnosed as tuberculous, before any symptoms or physical signs were present, all of whom ultimately developed undoubted infections, is sufficient in itself to prove the soundness of the method. But since such varying results are given by different observers, it is certain that some more fool-proof method is necessary for ordinary use.

I would suggest that the thing to aim at is the macroscopical rather than the microscopical test. Agglutination and haemolytic experiments, even if they are qualitative rather than quantitative, are more satisfactory and conclusive than numerical estimates of the opsonic kind, and I think that the haemolytic experiments of Dr. Emery and Captain Nesfield in the diagnosis of tuberculosis are most important and suggestive.

This is a bacteriological meeting, and therefore clinical work is, no doubt, somewhat at a discount, but, as a clinical physician, I feel that no discussion on the therapeutic use of vaccines is complete without emphasis being laid on the importance of clinical examinations in cases undergoing specific treatment. I hold that the attitude of Sir Almroth Wright in practically omitting clinical observation was justifiable, and indeed valuable, as a striking means of indicating the importance of specific treatment. He said, in effect, that he would treat bacterial infection and let the results of the infection look after themselves, and he has had, as we all know, an exceptional measure of success; but we do not all stand on the same plane as Sir Almroth Wright, and ordinary people will not get successful results unless they regard vaccine-therapy as a means of treating the cause of disease, and recognize that the effects and contributory causes must be removed by clinical means.

To take a simple instance in which I am personally interested: The importance of oral sepsis as a cause of disease is now generally recognized, but the prevalence of gingivitis, which is the preliminary stage of pyorrhoea alveolaris, has to be personally investigated to be understood. The presence of a septic focus on an inflamed surface connected with the mouth, to which many infected articles have access, and the fact that this septic focus is placed at the inlet of the alimentary canal, makes the importance of the condition apparent. Gingivitis appears to be in the majority of instances an infection with the *Streptococcus brevis* of the mouth, though doubtless other organisms may be involved. The condition is notoriously intractable by ordinary clinical methods, and it is obviously a case in which vaccine-therapy is desirable; vaccine-therapy, however, will have no effect except on the organisms which are actually within the tissues. It is clear, therefore, that all foreign substances, such as tartar, dead and loose teeth, which harbour bacteria and are out of range of the vaccine, must be systematically removed by clinical means. It is, in my experience, extraordinarily difficult to get thorough local treatment carried out, but it is essential to permanent success in the treatment of the condition itself or of the metastatic infections which may arise in consequence of it.

The doses employed in vaccine-therapy vary very much

in the hands of different workers. The followers of Sir Almroth Wright sometimes employ doses as small as $\frac{1}{100000}$ mg. of tuberculin (bacillary emulsion), and their maximum dose does not often, I think, exceed $\frac{1}{1000}$ mg. Dr. Camac Wilkinson gives doses of 5 mg., five thousand times as much as Wright's maximal dose, and both claim and achieve good results. The difference is prodigious; if such differences obtained on the question of the dosage of tincture of digitalis, they would vary between 15 minims and 7½ pints—nearly a gallon. In the manner of gonococcal infections, Wright's school gives doses varying between 2½ million and 50 million. Dr. Allen gives 200 million to 1,000 million. Such differences of opinion must be very confusing to persons who are not experts. We can only conclude that the correct doses of tuberculin and other vaccines have yet to be estimated.

In the case of vaccines other than tuberculin, consideration of these facts brings one directly to the question of the standardization of vaccines. In the first place, there is no doubt that the efficiency of different strains of organisms employed as vaccines is very variable, and it is and must remain difficult to allow for this factor in attempts to devise a universal method. But the method of counting ought to be agreed upon, for the one adopted in any given case will seriously modify the supposed strength of any vaccine. My friend Dr. Braxton Hicks has carried out a large series of comparative experiments in the Pathological Laboratory at Westminster Hospital in methods of counting bacteria, and recently published his results.[1] He used Wright's method, the direct method of counting on the Thoma-Leitz slide, and a method of his own, namely, that of mixing the vaccine to be estimated with a standard suspension Gram-negative or Gram-positive bacteria, and counting a Gram-positive vaccine against a Gram-negative suspension, or vice versa, the standard being estimated by the methods of dilution and culture employed in sewage estimations. Dr. Hicks tells me that, with any vaccine estimated by all three methods, he has found Wright's always the lowest, his own the highest, and the Thoma-Leitz method between the two. He has found the numbers obtained by his method compared to Wright's as 4 to 3. Probably this offers at any rate a partial explanation of the comparatively small doses successfully employed by Wright's school.

This does not explain the small dose employed by the same school in tuberculin treatment, since the tuberculins are estimated gravimetrically. Wilson and Charles Dickson[2] have recently advocated the gravimetric method of standardizing other vaccines, which is one which certainly ought to be considered in any standard scheme, as it has the great advantage of being macroscopic. I regret to have been unable as yet to try it.

With regard to tuberculin, I have to confess myself as an advocate of the small dose. I have recently had some instances of the value of small doses in the treatment of phlyctenular ulceration, in a series of cases treated at Westminster Hospital in conjunction with Mr. Brooksbank James.

In one case, stated to date "from birth," lacrymation was relieved; in the second intense conjunctivitis was relieved and an opacity reduced in size; in the third, with an inflamed sclerotic and corneal opacity, the pain was relieved; in the fourth, sclero-keratitis and cloudy cornea, there was apparently perfect cure; in the fifth, with keratitis of ten years' duration, there was reduction in size of the nebula, and an ophthalmic report of great improvement.

In the first three cases $\frac{1}{200000}$ mg. (bacillary emulsion) was never exceeded, in the fourth and fifth $\frac{1}{100000}$ mg. was reached. The sixth case is one of bad phlyctenular ulceration, with corneal opacities right and left of years' duration in a girl of 13; she has been under treatment a year, and can be kept fairly well; she has doses of $\frac{1}{250000}$ mg., and if the dose is increased to $\frac{1}{200000}$ mg. she gets a recurrence which lasts a week. Eye cases have the advantage that the results of treatment are readily estimated at sight, and with these experiences I am unable to overcome my sense of the risk of large doses.

I should like to demonstrate a modification in apparatus suitable for the preparation of vaccines in small quantities, such as are required for individual cases, for which, as a rule, test tubes are used, which have one or two disadvantages. They are wasteful, which is a matter of some importance in laboratories where economy has to be closely considered, and there is also a mechanical disadvantage. After introducing the vaccine the test tube is heated and drawn out into a long nozzle and the nozzle is sealed up. After shaking, the end of the nozzle containing a few drops of vaccine is cut off and reserved for standardization, and the test tube sealed again before the vaccine is sterilized. The difference between the calibre of the test tube and of the nozzle is considerable, and if a sufficiently long nozzle is left to allow of the test tube being sealed again easily after the extremity is cut off, masses of bacteria sometimes accumulate in the nozzle, from which they are not dislodged in the shaking process, and thus are not properly broken up.

The test tube may be replaced by a modification of Wright's recurved blood capsule made on a large scale. A long piece of wide glass tubing, with an outside diameter of about 7 mm. and an internal diameter of 6 mm., is cleaned and sterilized by heating to a sufficient temperature in the blow-pipe flame. The tube is heated again at a point 10 cm. from the first spot and again drawn out. The tube is cut in the middle of the drawn out part, and the diameter here should be not less than 1½ mm.

A capsule is thus prepared with a body 10 cm. long and with each end drawn out into a capillary extremity. One extremity is gently heated in a Bunsen flame at a point 2 cm. or 3 cm. from the body of the cylinder, and bent into a curve till the two limbs are at right angles to one another. It is then heated again at a point 2 cm. or 3 cm. from the extremity and bent in the opposite direction to rather less than a right angle, so that a Z-shaped curve is made. The curves must be gradual, so that the lumen of the tube is not unduly compressed. The vaccine is prepared in the ordinary way by rubbing the culture off agar slopes into salt solution. The liquid is withdrawn into the capsule as follows:

The agar tube containing the salt solution and the roughly suspended organisms is held in the left hand in a position as nearly as possible horizontal. The capsule is held horizontally in the right hand with the straight end pointing to the right, and the vertical limb of the curved end pointing upwards. The open extremity of the curved end is now introduced into the liquid which passes by capillarity up the tube, and when it gains the first curve, the tube is converted into a siphon, and the fluid accumulates in the capsule. The capsule must be held so that the straight extremity points slightly upwards, so that no fluid enters its straight capillary end, so as to obstruct the passage of air. A capsule of this size easily holds 3 c.cm. of vaccine with ample room for shaking, and this quantity is generally sufficient for an individual case. When sufficient fluid is collected, the straight end of the capsule is sealed in a Bunsen flame. The curved end is now cut off and sealed so as to leave a short nozzle, and the same is done afterwards with the straight end. It is easy to leave a nozzle of such a length that it can be cut off with sufficient liquid for the purpose of standardization, but without leaving a kind of backwater in which masses of organisms can accumulate. After agitation one end is cut off for standardizing, and the tube is sealed up again, while the opposite end remains to be cut off after the vaccine has been sterilized, to withdraw a sample for testing the sterility.

The organisms in common use which give the greatest trouble to break up are some of the streptococci and some diphtheroid bacilli. With regard to streptococci, it has long been known that a trace of sodium hydrate added to the vaccine sometimes assists in breaking up; about 100 c.mm. of normal NaOH to 3 c.cm. of salt solution is generally sufficient. A mechanical contrivance which is of considerable assistance is the following:

The nozzle of the capsule is cut off and a strand of platinum wire 3 cm. or 4 cm. in length is twisted into a spiral, sterilized, introduced into the capsule, and fused into the nozzle. This acts something like an egg-whisk, and is more efficient than the addition of glass beads in breaking up the clumps of organisms.

Diphtheroid bacilli will break up readily on the addition of a trace of hydrochloric acid to the suspension. If the vaccine is estimated by standardizing against blood a very minute trace of acid only must be used, or the blood corpuscles will be haemolyzed.

For bacteria which resist these methods prolonged shaking should be carried out, and the capsule should then stand in a vertical position until the larger masses accumulate in the lower nozzle; this may then be cut off and rejected, and shaking should be renewed. Sufficient organisms for a weak vaccine will often be found to be present in the residue.

These small modifications have been found of service in the course of routine laboratory work, and their use in suitable cases will result in some saving of time.

REFERENCES.

[1] BRITISH MEDICAL JOURNAL, April 27th, 1912.
[2] *Journal of Hygiene*, May, 1912.

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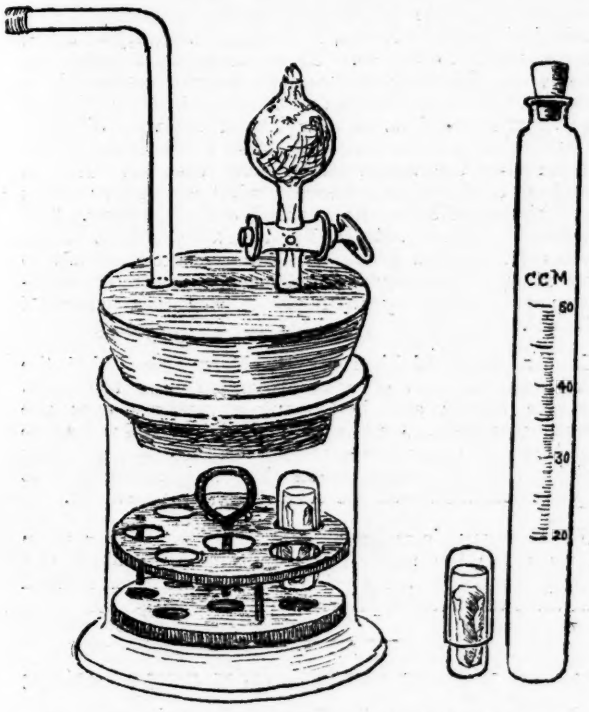
THE USE OF A GRAVIMETRIC METHOD IN THE STANDARDIZATION OF VACCINES.

I THINK it will be admitted by all that the expression of the dosage of vaccines, like that of all actual remedies, in terms of weight is the most natural and scientific method. Methods in which the dose is arrived at by counting the number of bacteria present in a suspension have been almost universally employed in this country and in America, due to the influence of the pioneer of vaccine



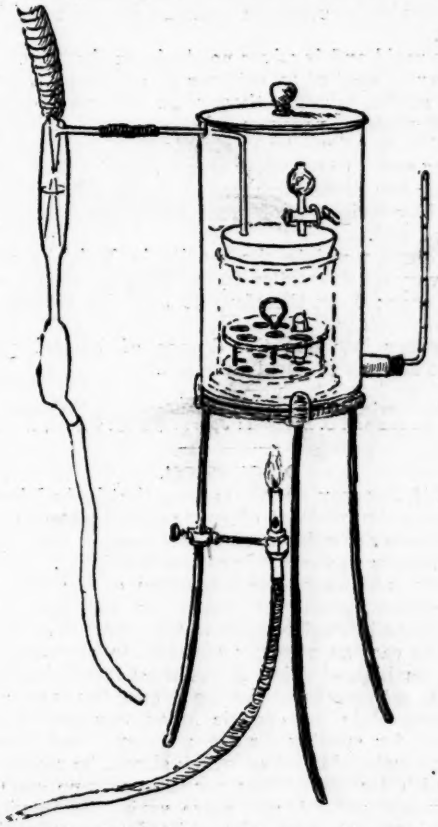
therapy, Sir Almroth E. Wright. The technique of the teat and capillary pipette in the Laboratory of Clinical Pathology has caused the balance to vacate the honoured position which it is accustomed to hold in other scientific laboratories. The fact that the balance is still employed in the standardization of one of the most active remedial agents known—tuberculin—inspires some of us with the hope that it will become again the most delicate and treasured instrument in the bacteriologist's armament and that the standardization of vaccines on a gravimetric basis will produce the uniformity of dosage which is so much to be desired. What has commended the counting method to the clinical laboratory worker of this country is the rapidity in which the standardization can be effected. Absolute accuracy has never been claimed for it, and the work of Leishman and his colleagues has shown that the error in the determination may be very considerable if the original method of Sir Almroth Wright is used. The following factors appear to interfere with the accuracy of the method: (1) The difficulty in securing a perfect film in which the ratio of germs to cells shall be constant throughout, (2) the clumping of bacteria, (3) the bacteriolysis and autolysis of the bacteria, (4) the fact that during the process of fixing and staining a proportionately larger number of the bacteria than of the more bulky red cells may be washed away.

In a recent paper[1] Dr. Charles Dickson and I attempted to obtain a correlation between the dried weight and the number of bacteria present in a suspension in distilled water. The number of bacteria present was estimated by Wright's method. This probably resulted in an underestimation of the bacteria, since in the blood films the red blood cells occupied about one-third of the superficial area of the slide, and probably concealed many of the bacteria. But as our aim was to ascertain what weight of dried bacteria was represented by the number of bacteria, as determined by Wright's method, our object was attained. In the following table a summary of the results obtained by us is given:

Organism.	Dose expressed in Millions.		Dose expressed in Milligrams.	
	Minimum.	Maximum.	Minimum.	Maximum.
Streptococcus (3,400 million=1 mg.) ...	6.8	68	0.002	0.02
Gonococcus (4,500 million=1 mg.) ...	45	900	0.01	0.2
Meningococcus (3,000 million=1 mg.) ...	300	900	0.1	0.3
M. melitensis (14,000 million=1 mg.) ...	700	1,400	0.05	0.1
B. coli (6,400 million=1 mg.)	16	240	0.0025	0.0375
B. typhosus (8,000 million=1 mg.) In treatment	100	250	0.0125	0.03125
In prophylaxis	500	1,000	0.0625	0.125
B. pyocyaneus (3,400 million=1 mg.)...	34	1,020	0.01	0.3
B. pneumoniae, Friedländer (4,400 million=1 mg.)	44	...	0.01	...
Staphyloccocus aureus (3,000 million=1 mg.)	150	900	0.05	0.3
B. tuberculosis	...	...	0.00005	0.005

It is now my practice to standardize all vaccines by the gravimetric method. It has been found that by the following procedure the standardization can be effected more accurately and quite as rapidly as by the counting method:

A piece of weighed platinum foil (1.5 in. by 1 in.) is sterilized by heat and held by a pair of Cornet's forceps. Over the centre of the upper surface the growth from a culture on agar of the micro-organism is smeared. To avoid contamination of the foil it is useful to cover the pans of the balance with discs of platinum foil, the one on which the rectangular piece is to be placed being sterilized by heating in the flame. This is not



necessary if the rectangular foil is rolled up and placed in a tubular glass weighing bottle, but I have an impression that more accurate and constant weighings can be obtained without the use of a glass weighing tube. The platinum foil on which the growth has been smeared is now rolled up and placed in a short sterile glass tube covered over with a loosely fitting cover and then placed in the desiccating chamber. This* consists of a

* Made by Baird and Tatlock.

short round glass jar fitted with a rubber bung and contained in a copper vessel, kept at a temperature of 42° C. Penetrating the bung are two glass tubes, one of which is connected by a short piece of glass tubing to a Sprengel pump, whilst the other has a bulb filled with cotton-wool, which acts as a filter when air is admitted by a stopcock to the desiccator at the end of the experiment.

It has been found that ten minutes in the desiccator suffices for complete desiccation. The platinum foil is then weighed, and the dried bacteria are made into a uniform suspension by spreading out the foil in a sterile porcelain dish and rubbing it with the rounded end of a sterile hard glass tube which has been moistened with a drop of saline solution, more salt solution being gradually added. The same procedure is used as in the preparation of an emulsion of tubercle bacilli, but the time required is only two to five minutes. The emulsion is placed by means of a sterile pipette into a graduated test tube (the old form of Stokes's tube is convenient), and saline solution used for washing out the dish is added until the necessary degree of dilution is obtained. In most cases this procedure is unnecessary, since a uniform emulsion can usually be obtained by closing the Stokes's tube containing the foil and diluting fluid with a rubber cap and giving it a few shakes after it has been a few minutes in the sterilizer. The tube is then placed in a Hearson's water-bath sterilizer at a temperature of 55° C. to 60° C. for one hour, and is afterwards placed in bottles or bulbs in the usual way.

I find that in practice the following amounts may be regarded as suitable doses: Streptococci $\frac{1}{3\,0\,0}$ mg., *B. coli* $\frac{1}{3\,0\,0}$ mg., staphylococci $\frac{1}{1\,0\,0}$ mg.

Probably in no condition are the results of vaccine therapy so successful as in the cases of infection with *B. coli*. I have used as an initial dose $\frac{1}{3\,0\,0}$ mg. of dried colon bacilli, and the results have been exceedingly good. Most of these have been cases of cystitis and pyelitis, but one was a case of gangrenous appendicitis, and here the vaccine probably saved the patient's life.

The advantages of the gravimetric method are:

1. That with a sensitive balance constant results are obtained.
2. It is rapid and involves no strain on the eyes.
3. It can be applied to cultures of all bacteria growing on solid media, whether these form uniform emulsions or not—for example, vaccines of streptococci, *Micrococcus catarrhalis*, *B. tuberculosis*, streptothrix and diphtheroid organisms can be accurately standardized. The addition of a pinch of sodium bicarbonate to the diluting fluid (as suggested by Wright) is of great assistance in emulsifying the last named.

Small clumps vitiate the results in Wright's method, but to a very much smaller extent the gravimetric estimation of dosage; it can therefore be used for standardizing sensitized vaccines.

4. It brings into line the dosage of bacterio-proteins with that of other medical remedies.

REFERENCE.

[1] A Rapid Gravimetric Method of Standardizing Vaccines, by Drs. W. James Wilson and C. Dickson: *Journ. of Hygiene*, xii, I, 49 (1912).

DISCUSSION.

Dr. W. M. CROFTON (Dublin) stated that he had found the haemocytometer method of estimation most convenient. He was further of opinion that the opsonic index yielded uniform results in the hands of one individual. As it was carried out it indicated the fluctuation of the most labile content of blood serum, but not of the more specific antibodies. He had therefore come to the conclusion that if the dose of the vaccine was only sufficient to keep the opsonic index at its highest point, it was often insufficient to produce sufficient specific antibodies to cure the case, and for this reason, while the opsonic index was useful for diagnosis and for spacing the doses, it should not be used to indicate the size of the dose which should be reached. He agreed with Dr. Eyre that since there was no such thing as a standard patient, and since variations in virulence and in the strains of microbes existed, no standardization was at present possible.

Dr. CRUICKSHANK (Dumfries) expressed dissatisfaction with the present attitude of bacteriologists toward vaccine treatment. It was necessary, before any discussion on the standardization and control of vaccines could be of use, that a clear idea of the *rationale* of vaccine-therapy should be settled. Vaccine-therapy had been introduced as a method of treatment for chronic infections, whereas it was now being used in acute infections, where the patient was obviously very ill, and was presumably suffering from the effects of the infecting organisms.

Professor ERNEST GLYNN (Liverpool) said that Leishman found that typhoid vaccine made from virulent cultures was no more effective than less virulent cultures. The speaker had found that counting in a modified Thoma-Zeiss chamber gave higher and more accurate results than Wright's method.

Dr. JAMES RITCHIE (Edinburgh) drew attention to the importance of Dr. Freeman's statement that while it was desirable always to follow the course of a vaccination by serological methods, it was, in the great majority of cases, impracticable to do so. This practically meant that usually the effects of any dose could only be estimated by its clinical effects. Taking into account the very different views put forward as to dosage, and the very different methods used for the standardization of vaccines, it was evident that the vaccinator in every case set out on a voyage of discovery, not knowing what adventures might befall him, even before he was well out of harbour. The speaker could bear out Dr. Freeman's remark as to the profound ignorance displayed by many practitioners who were using vaccine-therapy, and considered that the application of vaccine by such people was little short of criminal.

In the course of his reply Dr. EYRE agreed that the gravimetric method of standardization gave the most accurate results and had many advantages over the counting methods. On the other hand, he could not agree that there was any correspondence in the doses given by weight, any more than when the other methods were applied. He instanced the case of *Bacillus typhosus*.

Dr. FREEMAN, in reply, expressed himself with caution in regard to the method of standardizing vaccines, and merely pleaded for a control by the opsonic index method whenever this became possible.

ON THE DIRECT CULTIVATION OF TUBERCLE BACILLI FROM TUBERCULOUS TISSUES.

By JOHN CRUICKSHANK, M.B., Ch.B.,
Pathologist to the Crichton Royal Institution, Dumfries.

WITHIN recent years numerous methods to facilitate the detection of tubercle bacilli have come into use. These so-called concentration methods depend for the most part on reagents which are capable of causing disintegration or solution of sputum, pus, or other infected material, while leaving tubercle bacilli contained in such material comparatively unaffected. By the employment of such agents tubercle bacilli present in large volumes of material can be brought together into small bulk and their detection by microscopical methods thus greatly facilitated. The more scanty the organisms the greater is the advantage of employing concentration methods. Of the various procedures which have been suggested, probably the most satisfactory and at the same time the most simple is the use of antiformin.

This reagent, introduced by Uhlenhuth[1] (1908), consists of a mixture of sodium hydrate and sodium hypochlorite in certain proportions, the latter varying somewhat in the various preparations now in the market. As originally used by Uhlenhuth, it consisted of sodium hydrate 7.5 per cent., with sodium hypochlorite in such amount that 100 grams of the mixture liberated 5.3 grams of chlorine gas. For ordinary purposes equal parts of liquor sodae chlorinatae (*B.P.*) and 15 per cent. sodium hydrate serve very well. Such a mixture produces rapid disintegration and solution of pus, mucus, caseous and other material, and destroys the majority of organisms, while it leaves tubercle and other acid-fast bacilli unaltered. The latter organisms, along with a certain amount of material which resists the solvent action of the antiformin,—pigment, granular detritus, etc.—are obtained in the deposit which comes down on centrifugalizing the mixture or on allowing it to sediment.

It has been demonstrated by Meyer,[2] Haserodt,[3] Goerres,[4] Hüne,[5] Boardman,[6] and others, that antiformin is of considerable service in the detection of tubercle bacilli in sputa which have yielded negative results with ordinary smear preparations. These workers have found that from 7 to 17 per cent. of sputa negative by the ordinary method were positive after the use of antiformin. Other workers have not found the number of positive results to be so high. The differences in the figures obtained are probably dependent on the classes of case which have been chosen for examination. Antiformin obviously will be of the greatest service where the series examined includes a large number of sputa with very scanty tubercle bacilli. It has been as an aid to microscopical diagnosis that antiformin has been for the most part employed, but the method is also applicable to the direct cultivation of the tubercle bacillus from tuberculous material. The disintegrating effect of antiformin on pyogenic organisms is very marked, and the sediment from treated material is, except for the presence of tubercle bacilli, usually quite sterile. It has been shown by Uhlenhuth that tubercle bacilli after antiformin treatment are still pathogenic for animals and capable of growth. By the use of antiformin Uhlenhuth obtained a number of direct cultures on glycerine agar. The latter medium is, however, not the most suitable for obtaining growth. Results of more satisfactory nature are obtained by the use of Dorset's egg medium, as the results of Park and Krumwiede[7] and of the English Commission have abundantly demonstrated. With this medium Brown and Smith[8] have succeeded in obtaining cultures by means of antiformin from 33 out of 35 positive sputa.

The importance of direct culture need not be emphasized. Not only are the difficulties attendant on animal injection and the fallacies which not infrequently arise in interpreting the results of injection avoided, but the saving of time and trouble is very considerable.

In this paper the method adopted in obtaining a number of direct cultures from various tuberculous tissues by the use of antiformin is given in detail, with observations on the cultural characteristics of the organisms thus isolated.

Method.—To the sputum, sediment from urine, or other fluid a 15 to 20 per cent. dilution of antiformin is added, and the mixture allowed to stand, solution being aided by occasional vigorous shaking. Where large volumes of sputum are to be dissolved stronger solutions of antiformin may be used with advantage, but the resulting mixture in any case should not contain more than 15 per cent. antiformin. (This is not necessary if the sediment is to be used only for microscopical purposes.) As a rule, with the majority of sputa one volume is readily dissolved by three or four volumes of antiformin, but the amount of antiformin added to any particular specimen depends on the consistency of the sputum. Complete solution can usually be obtained within a comparatively short time, depending on the nature of the material under treatment. One or two hours is usually sufficient for sputa, but longer may be necessary with more refractory material such as thick caseous matter. In the case of solid tissues such as tuberculous lung or gland the material is cut up into small fragments and rubbed up in a mortar with a small amount of antiformin; the latter is then poured off and the material again treated. In this way by repeated rubbing with small amounts of antiformin, the solution of a considerable amount of material can usually be obtained in a comparatively short time. Efficient antiformin should always produce distinct liberation of gas during the process of causing solution. The antiformin mixture is then centrifugalized and the sediment which results is washed twice with sterile water or saline solution. A microscopical preparation should then be made as a control to estimate the number of bacilli present in a loopful of sediment. The washed sediment is inoculated on various culture media, of which plain egg (Dorset),[9] glycerine egg (Lubenau),[10] and animal tissues (Frugoni)[11] have been found to be by far the most satisfactory. As will be referred to later, it is probably advisable in each case to use all three media.

Egg Medium.—This has been prepared as follows: Two or three absolutely fresh eggs are washed and the shells sterilized by washing with undiluted formalin. The shells are allowed to dry and the eggs broken into a sterile dish. The yolks and albuminous portions are thoroughly mixed by gentle stirring (frothing is to be avoided), and the mixture is strained through sterile gauze. The volume is measured, and to three parts of egg one part of sterilized 0.85 per cent. NaCl solution is added. In the case of glycerine egg, one part of 6 per cent. glycerine in 0.85 per cent. NaCl solution is added to three parts of egg. A few drops of alcoholic basic fuchsine (sufficient to give a distinct pink colour) are added, and the mixture placed in sterile tubes. (The addition of a colouring agent is of distinct advantage in aiding the detection of early or very scanty growth.) The tubes are then placed in the sloped position high up in the steam sterilizer and coagulated (three to five minutes). Care is necessary at this stage not to expose the tubes for any longer period than is absolutely necessary for coagulation, otherwise bursting of the medium results. A smooth surface should be obtained. The tubes are further sterilized by heating at 105° C. for twenty minutes on two successive days, all the valves of the autoclave being tightly screwed down before heating is commenced. To prevent drying of the medium on standing, glycerine bouillon may be added, and a small amount should be allowed to remain in each tube at the time of inoculation. The presence of moisture in the cotton-wool stoppers must, as far as possible, be avoided, otherwise, owing to the length of time during which the tubes are incubated, moulds may grow. After inoculation the tubes should be sealed with paraffin wax. Uninoculated medium is best kept in the ice-chest.

Animal Tissue Media.—This is prepared by Frugoni's method. Fresh rabbit lung or other tissue, after soaking for an hour in 0.85 per cent. NaCl solution, containing 6 per cent. glycerine, is sterilized by autoclaving at 120° C. for half to three quarters of an hour, and is then supported over the surface of 6 per cent. glycerine bouillon, so that the tissue surface is kept moist by capillary attraction and by condensation. Tissues obtained at *post-mortem* examinations may also be used; these are sterilized by boiling or by autoclaving. I have found that the preliminary treatment with glycerine salt solution is not necessary.

Results.

In all about sixty growths have been obtained, the majority from tuberculous sputa. Urinary sediments, cerebro-spinal and joint fluids, lung and gland tissues have also given successful results. A small number of failures occurred in the earlier part of the work, but these were attributable to faulty technique, such as too prolonged treatment with antiformin or insufficient washing. With increased experience almost uniformly successful results have been obtained.

Egg Media.—In the case of the glycerine egg media growth appeared in most cases in from twelve to twenty-four days, the amount of primary growth depending in general on the number of bacilli present in the inoculated material. This, however, has not always been the case, as in a number of instances scanty growths were obtained from material which, microscopically, showed abundant bacilli. It has not been determined whether this was due to variability in resistance of individual organisms to the action of antiformin, as seems most likely, or to the varying viability of the bacilli in the original material. Greater difficulty has been experienced in obtaining growths from the tissues of acute tuberculous cases (injected animals, for example) than from those of chronic tuberculosis. Sputa from cases of chronic phthisis generally have yielded rapid and abundant growths. The character of the cultures has depended on the amount of growth. Individual colonies appeared as white, firm masses, raised in the centre and markedly rigid, the edges having an irregularly scalloped appearance. More abundant cultures showed a raised growth, white and firm, becoming wrinkled as incubation was prolonged. Pigmentation has been observed in only a few cases, and in these only after a prolonged incubation or after standing at room temperature for some time. On plain egg medium there has been in the great majority of cases very scanty or no growth, and in no case has growth been obtained only on plain egg. Subcultures from glycerine egg on plain egg have grown fairly abundantly, though in most cases much more slowly than similar cultures made at the same time on glycerine egg. All the cultures have therefore been of the "eugonic" or human type. This agrees fairly closely with the results of Park and Krumwiede,[12] who have found that bacilli of the "dysgonic" or bovine type are seldom found in connexion with tuberculosis of the lungs in adults. The series of sputa which I have examined were found on examination of the records to be all from adults. In one case the cerebro-spinal fluid from a child of 3 months gave a growth of human type. Dr. Gilmour, Gartloch Asylum, who has also used the method, has, in addition to sputa, obtained growths from a tuberculous joint and a tuberculous knee. No bacilli were demonstrable microscopically, even after prolonged examination, in these fluids after treatment with antiformin.

Animal Tissues.—In the case of animal tissues growth is usually more rapid, appearing as a rule in from seven to fourteen days. In a few cases growth was visible in three to five days. The characters of the cultures on animal tissues were practically the same as those on glycerine egg media, though in general much softer and more moist. It is worthy of note that although the growths

on the tissues were in direct contact for considerable periods with the glycerine bouillon in the tubes, it was only in a very few cases and after prolonged incubation that growths on the surface of the bouillon resulted. On the other hand, the tendency of the cultures to grow on the condensation water has been very marked. In a number of cases the spread up the walls of the tube was so marked that the cotton-wool stoppers were contaminated. The superiority of the glycerine egg and the animal tissues over glycerine serum, glycerine agar, and glycerine bouillon has been abundantly demonstrated. Glycerine egg medium has been found to be for routine purposes the most reliable. It is to be noted, however, that media containing glycerine have been shown to be more suitable for the growth of the human type of bacillus than for the bovine (Moeller,[13] Beck,[14] Cobbett,[15] Park[6]). The latter apparently grows most readily in primary culture on media without glycerine, such as plain egg. For purposes of culture it is, therefore, advisable in all cases to inoculate tubes of each medium. The power of different tissues to afford a suitable medium for the growth of tubercle bacilli has also been tested. Lung tissue from the ox, sheep, dog, rabbit, and guinea-pig, liver and kidney of the rabbit and guinea-pig, human lung and thyroid gland, have all given satisfactory growth. On the whole, the lung of the rabbit, ox, and sheep have given the most abundant growths. The lung of the dog and the various tissues of the guinea-pig were the least satisfactory.

Summary.

1. By the use of antiformin direct cultures of *B. tuberculosis* have been obtained from over 60 sputa and other specimens of tuberculous material.

2. The marked superiority of egg and animal tissues over other media for the growth of tubercle bacilli has been substantiated.

3. All the cultures obtained have been of the human type.

4. The method is of very great efficiency for obtaining, directly and rapidly, pure cultures.

REFERENCES.

[1] Uhlenhuth und Zylander, *Berl. klin. Woch.*, 1908, lxv, No. 29. [2] Meyer, *Tuberculosis*, 1909, viii, 71. [3] Haserodt, *Hyg. Rundschau*, 1909, xii. [4] Goerres, *Zeit. f. klin. Med.*, 1910, lxx, 86. [5] Hüne, *Deut. med. Woch.*, 1909, xxxv, 1791. [6] Boardman, *Johns Hopkins Bulletin*, 1911, xxii, 269. [7] Park and Krumwiede, *Coll. Studies. Res. Lab., Dept. of Health, New York*, 1910, v. [8] Brown and Smith, *Journ. Med. Research*, 1910, xxii, 517. [9] Dorset, *American Medicine*, 1902, iii, 555. [10] Lubenau, *Hyg. Rundschau*, 1907, xvii, 1455. [11] Frugoni, *Cent. f. Bakt. u. Parasitenk.*, 1910, liii, 553. [12] Park and Krumweide, *Journ. Med. Research*, 1911, xxv, No. 2. [13] Moeller, *Deut. med. Woch.*, 1902, 718. [14] Beck, Festschrift zum Sechzigsten Geburtstage von Robert Koch, Fischer, Jena, 1903. [15] Cobbett, Report of Royal Commission.

DISCUSSION.

Dr. JAMES RITCHIE (Edinburgh) congratulated Dr. Cruickshank on having demonstrated so completely that direct cultures of the tubercle bacillus could be so uniformly obtained. He felt that some of the disadvantages of animal experiment could thus be overcome. In certain cases it was practically impossible to distinguish by inspection the granulomatous lesions produced by Pfeiffer's pseudo-tuberculosis bacillus from those due to the tubercle bacillus. He agreed with Dr. Cruickshank that some modification was necessary of the statement in the textbooks regarding the readiness with which the tubercle bacillus could be grown on glycerine media. It was only applicable to cultures which had long been accustomed to grow on such media. In primary and early secondary cultures the growth was relatively scanty, and, in the case of the bovine varieties, difficult to obtain at all.

Dr. HERBERT HENRY (Sheffield) asked Dr. Cruickshank what were his methods of obviating certain difficulties. The question of time limit—that is, the time during which the antiformin was allowed to remain in contact with the sputum or tissues—was important. Antiformin killed tubercle bacilli if left in contact long enough. Another difficulty resulted from the presence of sporing organisms, moulds and yeasts, which were not destroyed by antiformin until long after the tubercle bacilli were dead.

Dr. LEDINGHAM (Lister Institute, London) said that in the routine examination of sputum for tubercle bacilli at the Lister Institute of Preventive Medicine, the use of antiformin had only to a very small extent increased the percentage of positive results.

In his reply, Dr. CRUICKSHANK stated that in the majority of cases solution of sputum could be obtained in about two hours, provided that the mixture was well shaken. Sporing organisms had not been met with. The cultures had been obtained from 60 out of 70 specimens of sputum examined. He had only met with difficulty in obtaining growth in glycerine broth in the primary cultures. Old cultures grew fairly readily in the majority of specimens of glycerine broth.

A STUDY OF THE PYROGENETIC PROPERTIES OF B. TYPHOSUS.

By E. C. HORT and W. J. PENFOLD.

(From the Lister Institute of Preventive Medicine, London.)

To what is the fever producing effect of *B. typhosus* due? Is it due to the products of lysis or extraction of the organism, or is it due to the products of the action of the organism on the medium on which it grows? In typhoid fever in the human subject have we to deal with fever resulting from an anaphylaxis, the sensitization taking place during the period of incubation? Such are some of the questions on this subject which call for investigation. Unfortunately previous work on this subject has not given satisfactory results.

Sirotinin (1886), for example, observed the effect of intravenous injection of *B. typhosus* on the temperature of rabbits. At that time, however, the method of identification of the organism was not in a satisfactory state. And his observations on temperature were made so infrequently, the first observation being usually twenty-four hours after the injection, and in no case less than three hours, that he naturally missed the characteristic effects of injection of cultures of this organism on temperature.

Bouchard and Krehl (1895), again, described fever after injection of *B. typhosus* cultures in animals, and Krehl found falls of temperature in the case of doves, and recently Schittenhelm, Weichardt, and Hartmann have studied this subject in dogs. No one, however, of these observers took the precaution of freshly distilling the water used, or of using stored autoclaved water which had been autoclaved in sealed vessels immediately after distillation. This is absolutely essential in all experimental fever work, as we showed in 1911. We believe, therefore, that the pyrogenetic power of the *B. typhosus* has not been studied by any observer with due attention to the numerous precautions which recent experimental fever work of our own has shown to be necessary.

Our experiments with *B. typhosus* were all made on the rabbit. The injections were intravenous and the temperatures were taken in the rectum with Kew-certified thermometers. The thermometers were carefully tested from time to time.

As before mentioned, one of the first requisites of any experimental fever research is purity of reagents. The authors have shown that the injection of pure distilled water causes a fall of temperature; this is illustrated in the accompanying Chart 1. The fall varies, as a rule,

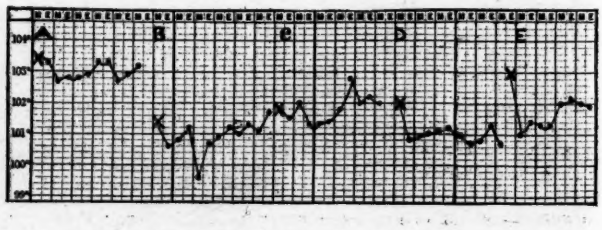


Chart 1.—Water controls.

TABLE I.

	No. of Animal.	Weight.	Water in c.cm.	Date of Injection.
A	260	1,297	12.0 1/108	March 25th, 1912.
B	277	1,690	18.0 1/93	April 17th, 1912.
C	273	1,406	18.0 1/78	April 16th, 1912.
D	X	2,080	52.0 1/40	(?) 1911.
E	Y	2,112	87.0 1/24	(?) 1911.

between 1° and 2° F. in rabbits, or more with larger injections. The injection of uninoculated nutrient broth in quantities of ½ to 1 c.cm. into rabbits of 2 kilos causes very slight rises of temperature. In quantities less than ½ c.cm. it does not produce fever even if injected in distilled water, so that the high temperature obtained by injection of small quantities of typhoid broth in distilled water must be due to either the *B. typhosus* itself or to products of its chemical action on the medium. The accompanying charts illustrate some of the points we wish to bring forward.

Temperature Curves showing the Effects of the Injection of Rising Quantities of B. typhosus Broth in Constant Quantities of Water.

Chart 2 shows the effect on temperature of injection of mixtures of *B. typhosus* broth with distilled water. The different curves represent the temperatures of different animals. All the animals were injected on the same day, each with one two-hundredth of its body weight of distilled water to which volume rising quantities of *B. typhosus* broth were added. The mixtures were injected intravenously as stated. It will be noted that the initial temperatures were not quite the same, but, making allowances for these differences, we notice that the highest temperatures of the series are in animals A and B, which received the smallest dose of typhoid broth. Further, the two largest doses of typhoid broth are characterized by curious notch temperatures, neither of which attains a great height. This curious notch type of chart we have found to characterize not only *B. typhosus*, but also *B. pyocyaneus*, *B. coli*, and other organisms, and is usually, but not invariably, due to a large dose. There is, perhaps, a depressant body present in addition to the pyrogenetic, or possibly it may be the effect of a larger dose of a single body.

Rabbits H and I, which had very similar doses, showed the most profound intoxication. L died in one hour, and H had a fairly marked fall. We note that the intoxication is not proportional to dose. Our next point is to show the effect of injection of constant quantities of *B. typhosus* broth and rising quantities of water.

If a series of animals be injected with a constant quantity of typhoid broth and rising quantities of water from 5 to 30 c.cm., we find that if the dose of typhoid broth be small the increased dilution gives increased fever. If, on the other hand, the dose of typhoid broth be a little larger, with smaller quantities of water, we find that more fever results than if no water be added. The largest quantities of water may cause a profound depression, and very frequently they result in death. These latter experiments we have already communicated to the *Journal of Hygiene* (1912), and on that account we do not propose to show illustrative charts. The general result is, however, that added water greatly increases the intoxication. Further, if the dose of typhoid broth is not sufficient of itself to cause intoxication as indicated by fever, the addition of water brings it out. This is also true if the water be given before the broth into the opposite ear vein, and does not, therefore, depend on *in vitro* extraction of the *organisms*.

Our next experiments deal with the source of the pyrogenetic agent in typhoid broth.

We wished to determine, in other words, if this pyrogen of *B. typhosus* broth is derived from bacterial cytoplasm or from the broth itself. If eighteen hours *B. typhosus* broth be centrifuged for half an hour and the deposit be made up to the initial quantity of broth by means of distilled water, and if then the pyrogenetic effect of this be compared with that of the supernatant fluid, it will be found that the two are about equally pyrogenetic. We have tested this by injecting small quantities of each in doses of water equal to $\frac{1}{200}$ of the body weight of the respective animals, and we found similar fever curves to result in each case. We deduce therefore from these experiments that the disintegrated organisms cannot themselves be the source of the pyrogen, though this experiment does not exclude the possibility of extractives from the living organisms being the pyrogenetic agents. We have endeavoured to remove the pyrogen from the bacilli by washing with either distilled water, saline, or broth, but none of these methods are effective, though it occasionally has happened that washing has appeared to diminish the pyrogenetic power. This is, however, exceptional. We are therefore inclined to believe that the pyrogen is not a bacterial extractive. We then examined the properties of *B. typhosus* pyrogen as regards heat and oxidation.

The pyrogen of the *B. typhosus* is, we find, markedly thermostable. It withstands autoclaving at 120° C. for twenty minutes. Both unheated *B. typhosus* broth and the heated gave rise to the same type of fever on injection into animals. This thermostability we also found in the case of the pyrogen of sterile water and saline which we showed to be due to a contamination capable of passing bacterial filters.

Oxidation, on the other hand, appears instantly to remove the pyrogen. If hydrogen peroxide, for example, be added to a centrifuged deposit of typhoid broth and then removed by the centrifuge and the deposit made up to its original volume with distilled water, we have found the deposit to be quite apyrogenetic. Simple standing in the cold room of the almost dry deposit has the same effect in diminishing the pyrogenetic power.

The immediate violent intoxication of *B. typhosus* as shown by high temperature, or collapse conditions with low temperature, is in sharp contrast to the results of injection of certain strains of *Staphylococcus aureus*. In the case of the latter organism the disturbances of temperature

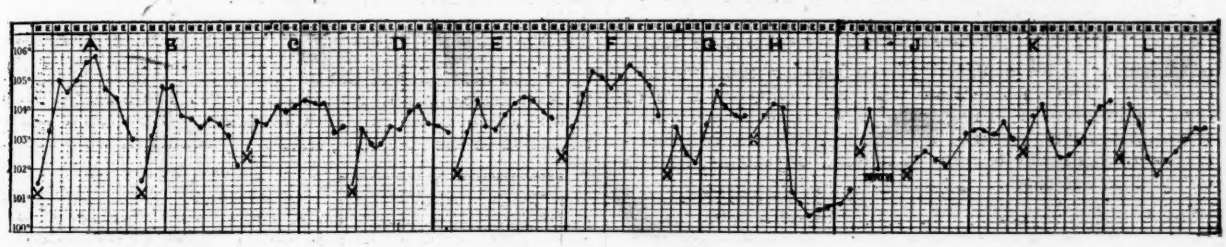


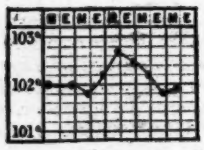
Chart 2.—Heated typhoid broth diluted (rising quantities of broth and constant quantities of water).

TABLE II.

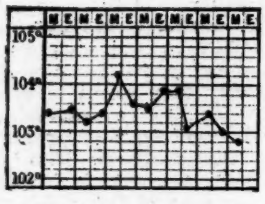
	No. of Animal.	Weight.	Heated T.B. Broth in c.cm.		Water in c.cm.		Date of Injection.
A	382	1,576	0.01	1/157,600	7.88	1/200	1912. May 5th.
B	385	1,576	0.05	1/31,520	7.88	1/200	"
C	380	1,520	0.10	1/15,200	7.60	1/200	"
D	383	1,463	0.20	1/7,315	7.31	1/200	"
E	381	1,800	0.40	1/4,500	9.00	1/200	"
F	378	1,406	0.50	1/2,812	7.03	1/200	"
G	377	1,463	0.60	1/2,438	7 31	1/200	"
H	375	1,800	0.70	1/2,571	9.00	1/200	"
I	400	1,800	0.80	1/2,250	9.00	1/200	"
J	379	1.913	1.00	1/1,913	9.56	1/200	"
K	374	2,250	1.30	1/1,730	11.25	1/200	"
L	376	2,476	1.50	1/1,650	12.38	1/200	"

All brindle rabbits.

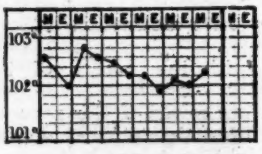
following injection of the dead organisms are negligible (see Chart 3). In the case of the living organism no effects are apparent till the next day. We have carried out a large series of experiments with different strains of these



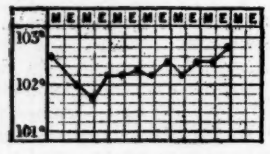
Date of injection, June 6th, 1912. 0.2 staphylococcus, 0.8 water, boiled.



Date of injection, June 17th, 1912. 1 c.cm. staphylococcus emulsion.



Date of injection, June 10th, 1912. 2 staphylococcic, 1 c.cm. water.



Date of injection, June 17th, 1912. 1 c.cm. thick staphylococcus, 2 c.cm. water.

Chart 3.—The staphylococcus emulsion in each case consisted of one standard loop of a twenty-four hours agar slope culture in 1 c.cm. of pure water.

organisms, and we constantly have found a most marked contrast. The intoxication with *B. typhosus*, alive or dead, is immediate in respect of fever or collapse, whilst the intoxication produced by certain strains of *Staphylococcus aureus* never occurs with dead organisms, and is delayed in the case of the living.[1]

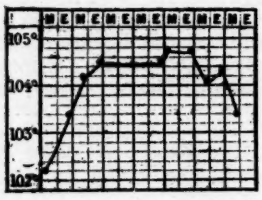
Anaphylaxis.

Friedberger and Mita (1912), in a paper on so-called anaphylactic fever, studied the effect of intravenous injection of foreign protein into guinea-pigs. They injected the protein, made up to 1 c.cm. volume in saline, into a vein in the neck which had been exposed by dissection of the animals after being fixed on an operation table.

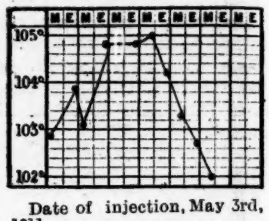
Friedberger and Mita distinguish the following doses of protein poisons worked with:

(1) The dose, for example, of serum just sufficient to give a fall of temperature, called the "limit of fall."
(2) The dose just smaller than the limit for fall which causes no disturbance of temperature. This they call the "upper constant limit."
(3) A still smaller dose giving a rise they call the "fever limit."
(4) The highest quantity less than that causing fever which has no effect on the animal's temperature they call the "lower constant limit."

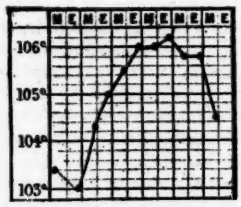
They state that these points are so affected in anaphylactic animals that one millionth of the quantity may suffice to produce any of the respective fever limit working with some varieties of proteins. We have shown elsewhere that the guinea-pig is a very unreliable animal for temperature observations,[2] and is in fact quite useless for the purpose. In Friedberger's experience it is quite easy to determine doses of certain proteins which give the effects indicated, and the establishment of these points, he states, has enabled him to discover anaphylaxis in animals at a very early date. Now Friedberger and Mita have shown in their paper (see Chart 1) that the method they used for intravenous injection of the guinea-pig caused a fall of over 1° C., although the material injected was only 1 c.cm. of normal saline; 1 c.cm. was the standard quantity of saline with which they worked, and still they establish these various "limits" by considering movements of temperature (see curve 2 in their paper) of less than half a degree. This, of course, is fatal to their deductions. Further, if the method of operation and the saline injection gave a fall of over one degree, it is difficult to see how they were able to obtain any of these limits with accuracy, and still more remarkable is it that they found no disturbance of temperature at all in so many of their animals. In uninjected animals, especially guinea-pigs, still more in animals that



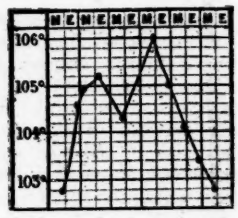
Date of injection, April 16th, 1912. 0.1690 c.cm. *typhosus* dilution.



Date of injection, May 3rd, 1911.



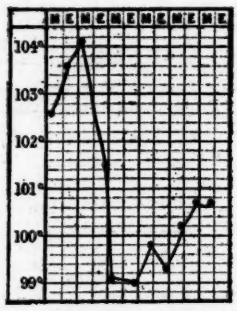
Date of injection, April 23rd, 1612. 0.49 c.cm. *Bacillus typhosus* broth.



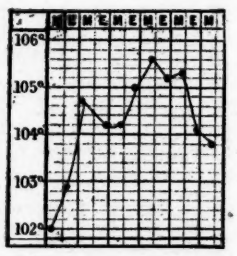
Date of injection, May 4th, 1912.

Chart 4.

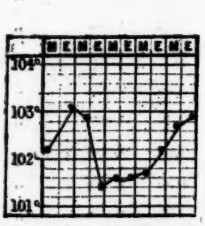
have had dissection of the neck done and injections given, it is practically never possible to get the straight line temperature these workers describe. We would draw attention to the fact, also, that no precautions were taken



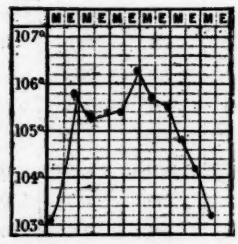
Date of injection, April 24th, 1912. 0.54 *typhosus* broth.



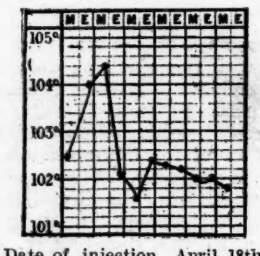
Date of injection, May 3rd, 1912.



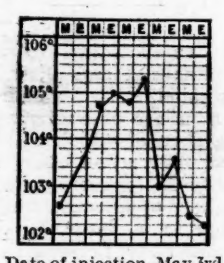
Date of injection, April 25th, 1912. 0.588 *typhosus* broth.



Date of injection, May 3rd, 1912.



Date of injection, April 18th, 1912. 0.1463 c.cm. *typhosus* dilution.



Date of injection, May 3rd, 1912.

Chart 5.

by giving control injections to ensure that the saline employed was apyrogenetic. That the apyrogenetic character of the saline must be demonstrated is admitted to be necessary now on all hands, even if the saline contains no organisms, as we have repeatedly shown. All these

[1] *Journal of Hygiene*, October, 1912. [2] Ibid.

objections being considered, we cannot take their evidence on anaphylactic fever as satisfactory.

We find, as stated by Friedberger and Mita, that small doses of the poison of, in our case, *B. typhosus*, give fever under conditions where large ones would give a fall. Alteration, on the other hand, of the conditions—for example, increase of the volume of diluent—completely alters the effect, as we have shown. Moreover, we frequently noticed sharp rises followed either by deep falls or by notches—that is, moderate falls and secondary rises of temperature. The regular simple temperature responses they have been able to obtain with all doses are quite unlike our experiments with *B. typhosus*. According to Friedberger and Mita, anaphylaxis to foreign protein can be demonstrated by the temperature method in spite of the fact that the stage of preparation has only lasted one day.

The accompanying charts (4 and 5) show the amounts of fever obtained in normal and prepared animals with the same dose of *B. typhosus* culture. Chart 4 shows that the fever response of the same animal to the same quantity of the same strain of *B. typhosus* was practically identical on first and second doses. It must be stated, however, that where on the first injection notch temperatures were obtained, the depressing effect was often absent on the second injection, perhaps suggesting immunity to the depressant body or effect. (See Chart 5.) We have been unable so far to obtain evidence that sensitization plays any important part in the causation of the long interval in typhoid fever between infection and onset of high fever. In short, in view of our experiments, with multiple injections of various organisms, we can at present find no justification for belief in anaphylactic fever in the sense hitherto described. The credence hitherto given to the conception has, we believe, arisen through non-recognition of the various fallacies that we have shown to be inherent to the study of experimental fever.

BIBLIOGRAPHY.

Sirotinin (1885), Die Uebertragung von Typhusbacillen auf Versuchsthiere, *Zeit. f. Hygiene*, Bd. 1, p. 465. Bouchard, cit. nach Rouguès, Substances thermogènes. Krehl und Mathes (1895), Versuch ueber Erzeugung von Fieber bei Thieren, *Arch. f. exp. Path. und Pharmac.*, Bd. 35, p. 222. Schittenhelm, Weichardt und Hartmann (1912), Eiweiss Umsatz und ueber-empfindlichkeit, *Zeit. f. exp. Path. und Therapie*, vol. x, H.3, p. 448. Friedberger und Mita, Ueber Anaphylaxie, *Zeit. f. Immunitätsfors. und exp. Therapie*, I Theil, Originale, vol. x, 1911, p. 216.

DISCUSSION.

Dr. JAMES RITCHIE (Edinburgh) said that he had been much interested in Dr. Penfold's results, as it seemed to him that they had a bearing on the possibility which many observers overlooked, that in certain circumstances pyrexia might be an indication of a beneficent reaction on the part of the organism. He drew attention to the fact that in the cases of some previous observers, who had studied temperature changes occurring after the introduction of pyrogenetic agents, insufficient attention had been paid to the technique of taking the rectal temperature in experimental animals. This point had been emphasized a good many years ago by Lorrain Smith. Dealing with the possibility of anaphylaxis playing a part in pyrexial manifestations, Dr. Ritchie asked Dr. Penfold whether he had tried a third injection given immediately after the effects of the second had passed off, in cases when the second injection had produced a rise of temperature equal to that caused by the first injection. If there were an anaphylactic element there ought to be no effect following such a third injection.

Mr. H. W. ARMIT (London) agreed with Dr. Penfold that there was no evidence, either in the data contained in the paper or in Friedberger's work, that the fever of enteric was due to an anaphylactic phenomenon, but he ventured to express the opinion that rabbits were unsuitable animals either to prove or disprove this suggestion. In his experience the anaphylactic shock in rabbits was limited to a sudden fall in blood pressure. At times paling of the ears and visible mucous surfaces could be seen, but he regarded it as comparatively rare to meet with those symptoms of shock, such as were almost always seen in guinea-pigs.

A CASE OF PYAEMIC ACTINOMYCOSIS WITH AN ACTINOMYCOTIC ENDOCARDITIS.

By GEORGE DEAN, M.A., M.B., C.M.,
Professor of Pathology, Aberdeen University.

IN the majority of cases of actinomycosis the spread of infection occurs by continuity. Spread by the lymphatics is denied by many authorities, who maintan that unless a blood vessel is perforated, extension of the disease occurs by continuity only.

Dissemination of the infective material by the blood stream with the formation of metastatic abscesses occurs, as a rule, only in the late phases of the disease.

Kanthack (1893) in recording such a case referred to the "extreme rarity" of the condition, and pointed out that it was interesting that in one of Israel's first cases the disease was of a pyaemic nature. At that time hardly any such cases had been recorded with the exception of Ponfick's (1882) two cases, in one of which an actinomycotic mass had burst into the jugular vein, and produced metastases in the heart, spleen, and brain; in the second case there were metastatic abscesses in the wall of the heart. A number of cases have been recorded since that time by, among others, Abée (1897), who reports and refers to a few similar cases, and by Harbitz and Gröndahl (1911) who give an account of 87 cases of actinomycosis which occurred in Norway. Although at the present time we cannot regard the pyaemic form as so rare as it was regarded in Kanthack's time, it is certain that in the majority of even the fatal cases the spread is by continuity.

In relation to the present observations, it is interesting to note that, though in a number of cases the pericardium and myocardium have been recorded as the sites of actinomycotic infection, in only one case have I been able to find a record of actinomytic infection of the endocardium.

Ponfick's second case, to which I have referred, had actinomycotic foci—the one in the right auricle and ventricle, the other in the left ventricle.

In Abée's case (3) there was present actinomycotic myocarditis. Abée also refers to a case of König's (1884), in which there were foci in the pericardium and myocardium.

Harbitz and Gröndahl (1911) also recorded several cases of actinomycotic pericarditis and myocarditis. Among the cases described by these writers is the only previously recorded case of actinomycotic endocarditis to which I have been able to obtain a reference. In their group of pulmonary cases they describe Case VIII as having, with other lesions, "endocarditis acuta (actinom.) aortae cum aneurysm." It is to be regretted that this record is so brief, as it leaves considerable doubt as to the exact nature of the condition.

The primary site of invasion of actinomycosis is often difficult or impossible to determine. From an analysis of 87 cases Harbitz and Gröndahl deduced the following figures:

Cervico-facial form	45 per cent.
Pulmonary	23 ,,
Intestinal	30 ,,
Skin	2.3 ,,

Out of 26 abdominal cases, 14 cases (9 certain, 5 probable) had their origin in the appendix, which, as will be seen, appears to have been the primary site of the infection in the case about to be recorded. In none of these cases could a primary infection of the mucous membrane be found.

Case of A. S., Farm Servant, Aged 22, Admitted to the Royal Infirmary on July 9th, 1910.

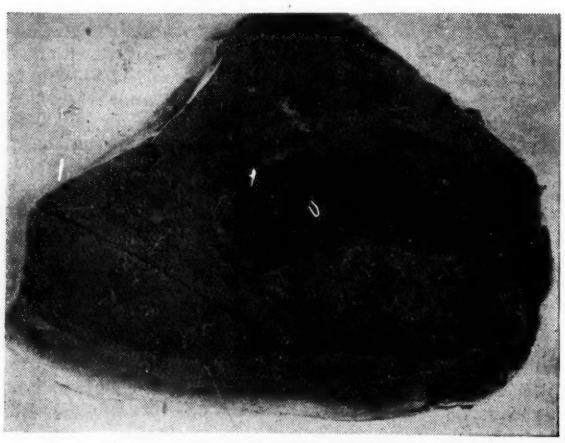
A gangrenous appendix was removed by Dr. Fred. Smith, to whom I am indebted for the specimen of the appendix. The patient was dismissed on July 22nd, 1910, and though relieved was never really well, and was readmitted on October 24th, 1910. An abscess of the chest wall was opened on December 6th, 1910. On January 31st, 1911, an abscess of the right lung having developed, pneumotomy and scraping were performed after partial excision of three ribs. The patient died on April 12th, 1911.

Autopsy.

The autopsy was made on April 12th, 1911. The cicatrices of the incisions of the appendix and other operations were seen. Two of these, over the region of the right lobe of the liver, terminated in an area of superficial ulceration, with a diameter of at least 2 in., and which was covered with pus. A sinus ran from this in the direction of the liver. The tissues in this region were markedly oedematous and partially gangrenous, and of a greenish-blue colour. A probe could not be passed by the sinus into the abdomen. It was probably caught in the recent adhesions over the liver, which will be described.

The Heart.—There were opacities in the endocardium of the left ventricle. On the auricular aspect of the mitral, ¼ in. from the free margin, there was a small nodular vegetation about the size of a split pea, somewhat warty in appearance. There was slight arterio-sclerosis of the aorta.

The Lungs.—The right weighed 1 lb. 13 oz. The pleural cavity contained 2 oz. of blood-stained serous fluid with particles of fibrin. There were recent fibrinous adhesions, and the fissures were glued together with a fibrinous deposit. In the centre of the upper lobe was an abscess cavity the size of a walnut. Scattered throughout the rest of the lung were numerous nodules from the size of a millet seed to the size of a bean, and the majority of these, on section, were found to be small abscess cavities. All the abscesses contained yellow, foul-smelling pus. The left weighed 1 lb. 11 oz. There were recent fibrinous adhesions between the layers of the pleura. The lung was in a similar but less advanced condition to that of the right lung, the abscesses being on the whole smaller.



Photograph of section of liver with abscess.

The Liver weighed 5 lb. 6 oz. On the lateral surface of the right lobe was an area 2 in. by 1 in. covered by fibrinous adhesions, and the liver in this region was of a dark slate-blue colour, resembling the appearance described in the case of the corresponding area of the abdominal wall. After removal two sinus openings were observed in this from which exuded evil-smelling chocolate-coloured pus. The openings appeared to have been sealed off by the fibrinous adhesions. On section these were found to open into a large sharply defined cavity, 3 in. by 2 in., with a slightly honeycombed reticular structure containing liver débris and chocolate-coloured pus. Fatty infiltration of the liver was present, and a slight amyloid reaction was obtained.

The Spleen weighed 1 lb. 8 oz. and measured 8½ in. by 6½ in. Contained no abscesses. Proved to be a typical sago-waxy spleen.

Kidneys.—Right weighed 7 oz., left 8 oz.; no abscess present. Slight parenchymatous nephritis with waxy degeneration: "large amyloid kidney."

The Appendix Vermiformis, which had been removed at the operation, was obtained after the *post-mortem* examination. It was about 6 in. in length, about the thickness of the forefinger, and somewhat tortuous. The walls were greatly thickened, and the lumen, which was almost occluded in parts, contained hard faecal material.

Histological Examination.—It is unnecessary here to enter into a detailed description of the microscopical structure of the various tissues. The abscesses in the right lung and liver had the ordinary appearance of actinomycotic abscesses and nodules. By the Gram and Gram-Weigert methods the actinomyces could readily be demonstrated in large masses within the abscess cavities. The lung presented many appearances of great interest. A number of the vessels were found to be thrombosed, the thrombi containing the characteristic branching mycelial filaments. Many of the appearances suggested that the material within the vessel had come as emboli, several of these being found at the division of the vessels. There was evidence of interstitial pneumonia round about the foci, and at other parts the alveoli contained a fibrinous deposit. In this case giant cells were not a pronounced feature.

In the appendix no complete evidence of the actinomycotic origin of the process could be found, though a considerable number of sections, at different levels, were cut and examined. In certain of these Gram-positive filaments were present, but no rosette arrangement could be made out.

It is necessary to give a rather more detailed account of the appearances found in the endocarditis. The vegetation stained with haematoxylin and eosin showed wart-like masses, the surface parts of which consisted of a material partly homogeneous, partly granular. The bulk of this stained with eosin, but a few rounded masses took a deep blue haematoxylin colour. The eosin-stained areas were evidently homogeneous and granular fibrin, the deep blue, calcareous material. At the base of the vegetation there were numerous rounded, angular, and spindle-shaped cells indicating beginning organization. A few smaller foci of polymorphonuclear and mononuclear leucocytes were present. Stained by the Gram or Gram-Weigert methods, here and there throughout the vegetation, mycelial filaments were seen, and in certain parts definite rosettes were present. The clubs could be made out indistinctly in the haematoxylin section. Many of the calcareous points appeared from their outline to be degenerated rosettes. No other organisms were present.

This condition, therefore, appears to be a case of true actinomycotic endocarditis. In a mixed infection it is conceivable that the condition might be due primarily to pyogenic cocci, but in the present case this is not probable, for if a coccal infection had occurred in a patient with resistance lowered as in the present case, it would, in all probability, have progressed. Neither in the endocardium nor in the other lesions were cocci present, though the occurrence of a few round actinomycotic bodies makes it difficult to be certain of this. This indicated that there had been no mixed pyaemic infection, even though the surface ulcers and the sinuses contained cocci.

The history of the case and the appearances found in the appendix make it probable that this was the primary focus of infection. It is possible that extension of the infection occurred by the portal vein to the liver and through the diaphragm or chest wall to the lung, and thence to the heart.

The chief point of interest in the case is, from its rarity, the occurrence of an endocarditis due to the actinomyces.

BIBLIOGRAPHY.

Bostroem (1891): Untersuchungen über die Aktinomykose des Menschen. *Ziegler's Beiträge*, Bd. ix, p. i. Kanthack. A. A. (1894): A Pyaemic Form of Actinomycosis, *Trans. Path. Soc. Lond*, vol. xlv, p. 233. Abée, C. (1897): Drei Fälle von tödtlich-verlaufener Actinomykose, *Ziegler's Beiträge*, Bd. xxii, p. 132. Harbitz, F., and Gröndahl, N. B. (1911): Die Strahlenpilzkrankheit (Aktinomykose) in Norwegen, *Ziegler's Beiträge*. Bd. 1, p. 193. König, A. (1884): Ein Fall von Actinomykose, Inaug. Diss. Berlin, ref. Abée.

DISCUSSION.

Dr. BEATTIE (Sheffield) raised the question whether the sago-waxy spleen was produced by a pyogenic coccus or by the actinomyces. Professor G. DEAN replied that he was satisfied that it was actinomyotic in origin. He regarded the ray fungus as a pyogenic organism. There were records of absolutely pure cases in which an amyloid spleen developed.

THE VARIATIONS IN THE MORPHOLOGICAL CHARACTERS OF BACTERIA AND THEIR REACTION WITH SUGARS UNDER DIFFERENT CONDITIONS.

(*Abstract.*)

DRS. J. M. BEATTIE AND A. G. YATES (Sheffield) read a paper on the above-named subject. Dr. Yates spoke first. He said that in order to test the value of the sugar reactions in the classification of streptococci, 38 strains were used. These were derived from various sources. They were all inoculated into animals, and on the basis of their pathogenic effects were divided into the three following classes:

1. Those producing septicaemia.
2. Those producing rheumatic arthritis.
3. Those producing no effect.

In class 1, producing septicaemia, there were 15 strains. On examining their sugar reactions, 9 distinct varieties were found, and, adopting the classification of Gordon, this group contained representatives of no less than 4 of his varieties — namely, *S. pyogenes*, *S. salivanus*, *S. augmosus* and *S. faecalis*.

In class 2, producing rheumatic arthritis, there were 13 strains. The sugar reactions of this group showed 6 varieties. Of these, one corresponded to *S. faecalis*, three to *S. salivanus* and its variants, and two did not correspond to any of Gordon's varieties.

In class 3, producing no effect, there were 9 (? 10) strains. According to their sugar reactions, there were 5 distinct varieties, including *S. salivanus*, *S. augmosus*, and 3 strains which did not correspond to any hitherto described variety. They could not help concluding from these results that the sugar reactions were of little value in the classification of streptococci, as they give us no idea of the probable pathogenic effect of the organism in question. Further, they had found that the sugar reactions of the individual strains of streptococci were not constant. This was tested in two ways: (1) after repeated subculture and (2) after passage through animals.

1. After repeated subculture. The results here were fairly constant, but in every case the inulin reaction was modified, becoming positive when it had been negative, or vice versa.

2. After passage through animals. Five strains were used. In the first the clotting of milk became negative. In the second the mannite reaction became positive. In the third the raffinose and salycin reactions became negative and the power of clotting milk was lost. In the fifth all the reactions were changed, except raffinose and acid production in milk. The conclusion which they were compelled to draw from these results was that the sugar reactions of an organism depended more upon the environment in which it had been living than upon its inherent properties.

They had found also that it was possible to modify the morphological appearances of certain organisms by altering the conditions of growth, especially in relation to time and temperature.

In working with the necrosis bacillus, they found that when grown on agar at 37° C. for twenty-four hours, the cultures consisted entirely of short oval bacilli. If, however, examined after only four hours' growth, the culture contained very numerous long filaments.

Exactly the same thing was found on examining cultures grown at room temperature. In forty-eight hours the cultures consisted almost entirely of long filamentous forms. If examined, however, after four days' growth, the filaments had all broken up, and nothing but short, almost coccal, forms could be seen. Cultures of the typhoid coli group were found to possess the same tendencies, but in a less marked degree. Long filaments in many cases were found in the early growths which disappeared if incubation was continued. Moreover, the bacilli in the early growths were longer than those in the older cultures.

It seemed clear, therefore, that the long filaments often found in cultures of these organisms represented an early stage of development, the filaments subsequently breaking up into bacilli. One bacillus, however, with which they had worked showed changes of an entirely different character. When grown at 37° C., long thick filaments with irregular staining were produced; if incubation was continued, they lost these staining properties, became vacuolated and finally broke up. The same organism, however, when grown at room temperature produced a copious growth of short, deeply staining bacilli, with no trace of filament. They had not identified the organism, but it was probably non-pathogenic.

These observations suggest that in describing an organism its appearance under different conditions of temperature and after varying intervals of growth should be given.

DISCUSSION.

Professor George Dean (Aberdeen) said that these results were of great importance as indicating that examination of cultures should be made at shorter intervals than twenty-four hours. He agreed that the results obtained with the necrosis bacillus showed that many workers had been misled in regard to the difficulty in the isolation of this organism. He himself had been led in error in this manner. He had often regarded the filamentous forms as involution forms.

Dr. Penfold (London) referred to the work of Ainley Walker on the streptococci, and questioned whether the changes in the sugar reactions were really due entirely to environment. He asked the authors whether they had sufficient evidence that the individual sets of germs in which the changes in the sugar reactions had been observed were all derived from a single organism.

Dr. Beattie, in reply, stated that they were quite satisfied that they had been dealing with a single organism the characters of which had undergone changes.

OBSERVATIONS ON BROWNIAN MOVEMENT, WITH SPECIAL REFERENCE TO THE ANTHRAX SPORE.

(*Abstract.*)

Professor E. Emrys-Roberts (Cardiff) read a paper compiled by himself and Dr. S. B. Walsh. He said that he had made the observation that anthrax spores suspended in tap-water exhibited practically no Brownian movement. This led them to investigate the phenomenon, and incidentally to endeavour to elucidate some points in connexion with Brownian movement.

In their investigations they made use of *Staphylococcus aureus* and Indian ink granules as controls, on account of their approximate size to anthrax spores, and also because the one could be taken as typical of dead particles and the other of living particles, both exhibiting well-marked Brownian movement. First of all, they worked out the time factor, and at the same time to a certain extent the diluent factor, using tap-water, distilled water, normal saline, 0.5 per cent. formol, 4 per cent. formol, and 10 per cent. formol—all made up in normal saline fluid —as suspensory fluids. The results were noted at room temperature, and therefore to this extent they introduced the temperature factor. The results may be briefly stated as follows: Whereas *Staphylococcus aureus* and Indian ink granules were found to exhibit well-marked Brownian movement in each and all the suspensory fluids at the end of a week, the anthrax spores showed little or no movement from the commencement, although the anthrax bacilli which were present did show Brownian movement. The temperature factor was more fully worked out by repeating the same experiments at 37° C. and 54° C., the results in each case being identical with those obtained at room temperature.

In order to test the vital factor, *Staphylococcus aureus* and the anthrax spores were killed by heat, but this was found not to affect the results in any way. In the next place, the weight factor was investigated. By making a mixed emulsion of all three sets of particles in tap-water and centrifugalizing, a most striking result was obtained; the particles arranged themselves in three layers—black, white, and yellow—from above downwards, indicating that anthrax spores occupied an intermediary position between the Indian ink granules, which were the lightest, and the *Staphylococci aurei*, which were the heaviest. This inference was verified by pipetting off portions from each layer and staining them suitably. By suspending portions of the respective cultures and the Indian ink granules in small tubes, containing increasing percentages of glycerine in water, they were able approximately to judge their specific gravities, and the results bore out and confirmed those obtained by the centrifuge.

The viscosity factor was determined by using as the suspensory fluid glycerine and cedar-wood oil on the one hand and ether on the other. It was found that in cedar-wood oil and in glycerine Brownian movement was almost suspended throughout, whilst in ether the movement was somewhat increased in the case of Indian ink granules and *Staphylococcus aureus*, but there was no appreciable increase in the case of anthrax spores.

The osmotic factor was easily disposed of, since, whatever inferences might be drawn from a comparison between *Staphylococcus aureus* and anthrax spores, such inferences could not be maintained between *Staphylococcus aureus* and Indian ink granules. Next, in order to determine

whether the property apparently possessed by anthrax spores was unique or was shared by spores in general, they worked out the spore factor, using the spores of tetanus, *Rauschbrand* and *subtilis*. It was found that these spores in the order named exhibited an increasing Brownian movement in comparison with anthrax, thus demonstrating that the property of comparative immobility is common to spores in general, and apparently decreases with the resistance of the spores.

Finally they endeavoured, by passing currents of electricity of from 1 to 2 volts, to discharge any electric charge the particles might have been endowed with, and thus to arrest the movement, but without success. So far, therefore, they were able to determine: that the observations of the peculiar behaviour of anthrax spores were fully confirmed; that the relative behaviour of anthrax spores, *Staphylococcus aureus*, and Indian ink granules was not altered by length of time or by differences of temperature; that the supposition held that formol added to the suspensory fluid arrested Brownian movement was without foundation; that the vital, weight, osmotic, and electric factors played no part in the determination of the movement; that the viscosity factor bore out what one would expect from the physical standpoint; and, lastly, that the peculiar behaviour of the anthrax spores was to a certain degree shared by other spores.

The opinion advanced by certain physicists that Brownian movement is an indication of the molecular movement of the suspensory fluid does not appear to explain the conditions found. In the first place, it is exceedingly difficult to suppose that the vast multitudes of molecular forces required to set in motion a single one of the particles could act in unison; moreover, if the supposition were granted, the only way to explain the comparative immobility of the anthrax spore would be by suggesting that it was provided over the whole of its surface with ultramicroscopic spicules, which would tend to neutralize the effect of one set of forces by the bombardment directed on the spore surface of sets of forces acting in other directions. This supposition is far removed from their conception of the anthrax spore, as an exceedingly smooth, highly refractile body.

The more generally accepted theory, that Brownian movement is based upon unequal differences in surface tension between the particles and the suspensory fluid, was felt to be a more reasonable explanation. It was noted that in the case of *Staphylococcus aureus* and Indian ink granules, tea-cup attraction was well marked, especially in the latter, whereas in the case of anthrax spores the attraction to the sides, though not absent, took an appreciable time to become marked. Had the anthrax spores refused to exhibit tea-cup attraction, the authors would have been obliged to postulate a molecular arrangement of the spores counterbalancing the differences in density between them and the suspensory fluid, which postulate would have broken down, because suspensory fluids of different densities were employed. The potential energy present by reason of the differences of density at the surface of the Indian ink granules, as compared with the suspensory fluid, would appear to be converted into actuality by the supposition that the particles contain local impurities, leading to unequal differences of surface tension at different points of the particles. Similarly, variations in the thickness of the envelope of the *Staphylococcus aureus*, or differences of the protoplasmic content becoming manifest through the comparative permeability of the envelope, would be adduced to explain the unequal differences of surface tension necessary to convert the potential into actual energy and hence into Brownian movement. As regards the anthrax spores, on the other hand, a reasonable supposition to account for their comparative immobility and slow tea-cup attraction is to postulate their possession of an envelope so nearly perfectly uniform that no unequal differences of surface tension exist, and hence no conversion of potential into actual energy. It would further seem that the tea-cup attraction varies with the exhibition of Brownian movement, suggesting that particles in oscillatory movement move more rapidly to the side than particles practically at rest. The supposition that the anthrax spores possess an almost uniform surface is quite in keeping with the usual conception of these bodies, while the exhibition of Brownian movement on the part of anthrax bacilli again bears out the idea that structurally they resemble *Staphylococcus aureus*.

Finally, in order to put to the test the theory that the absence of Brownian movement in the case of anthrax spores was due to uniformity of surface, experiments were carried out with antiformin and hydrochloric acid as follows: 50 per cent. antiformin, incubated at 37° C. for forty-eight hours: result, pronounced Brownian movement of anthrax spores; 50 per cent. of antiformin, first at 100° C. for five minutes, and then at 37° C. for forty-eight hours: result, the same; 50 per cent. HCl at 37° C. for forty-eight hours, practically immobile; 50 per cent. of antiformin at 37° C. for twenty minutes: pronounced Brownian movement; 50 per cent. antiformin brought to boiling: result, the same. It will be noted that 50 per cent. of hydrochloric acid at 37° C. for forty-eight hours produced no effect whatever, while the exhibition of 50 per cent. antiformin for only twenty minutes at the same temperature produced results similar to those produced by much more intensive treatment with the same reagent. Antiformin was chosen because of its well-known property of attacking cellular material; and judging by the results obtained, which were to an extent corroborated by the inability to obtain growths from the spores so acted upon, this reagent would appear to have so altered the surface of the spores as to produce inequalities sufficient to bring into play the forces leading to the production of Brownian movement, and this result would appear to strengthen the theory that Brownian movement is due to unequal differences of surface tension between the particles in question and the suspensory fluid.

At the outset it was hoped that it might have been possible to have come across a suspensory fluid which would abolish Brownian movement, or else to have evolved a method of arresting it, since in counting vaccines by the Thoma-Zeiss method considerable difficulty is always experienced by reason of this phenomenon, but the results of their observations would appear to hold out no hopes in this direction.

The authors finally express their indebtedness to Mr. J. H. Shaxby for his interest and assistance.

ON THE BACTERIOLOGY OF AN INFECTIVE DISEASE OCCURRING IN RANGOON.

By Captain A. WHITMORE, I.M.S.

IT is with some reluctance that I venture to ask your attention for a few minutes while I present to you a very brief description of the bacteriology of what is, I believe, a hitherto undescribed infective disease. Under ordinary circumstances, it is probable that the suggestion that the disease is "new" to medical knowledge would suffice to rouse your keen interest, but in this case it so happens that the disease is at present, at any rate, only of local importance, and the infected locality (Rangoon in Burma) is so very far away that your interest should be only lukewarm. However, the knowledge that one of the greatest drawbacks to which a lonely worker in the East is subjected is his freedom from the remotest criticism of his fellow-workers has overcome this reluctance, and it is with the object of eliciting criticism that I put this brief note before you.

As the only account of the disease as yet published has but just appeared in the recent number of the *Indian Medical Gazette*, and has probably been read by none here present, it seems to me essential that before proceeding to the purely bacteriological portion of my subject, I should give you some idea of the disease itself. While Pathologist at the Rangoon General Hospital it happened that my attention was drawn to the occurrence of several true glanders infections in man. The importance of this subject from a public health point of view led to our special attention being directed to its detection, and while so engaged we were struck by the fact that, in addition to the few cases which bacteriologically were easily proved to be true glanders, were several others in which, though the manifest lesions appeared to be typical of glanders, yet our bacteriological findings did not confirm such diagnosis. At first we were inclined to attribute this bacteriological confusion to our own inefficiency, but later we discovered that these seemingly mistaken bacteriological results bore a striking resemblance one to another,

and that with this similarity in bacterial findings was associated a similarity of lesions—that, in fact, we were probably dealing with a disease distinguishable by both constant bacterial pictures and characteristic gross lesions. An experience of ten months has amply confirmed this view. The disease has been met with thirty-eight times in all. It is probably a septicaemic disease. Unfortunately, in all but one of our cases bacteriological examinations have not been undertaken until after death, but in such cases it is usual to be able to recover the bacilli from the heart's blood, and almost invariably they can be recovered readily from spleen cultures. In the only non-fatal case we succeeded in isolating the bacilli from the peripheral blood during the attack.

Associated with this septicaemia there are gross lesions in many organs. Generally these lesions occur in the lungs, and these lung lesions are strikingly similar in their acute form to those so commonly present in acute glanders of man. Some characteristic specimens of these lesions are shown in the museum. In addition to these pulmonary lesions, there are frequently gross lesions in other organs, such as the liver and kidneys. A grossly infected kidney is shown in the museum.

Upon the clinical aspect of the disease I do not propose to enter, except to say that clinically the disease may resemble glanders very closely indeed.

The bacillus causing the disease occurs in very large numbers in all the acute lesions, and may be seen in large numbers in smears taken from these lesions. It can be readily isolated in pure culture not only from the obvious lesions, but also usually from the spleen of infected persons, and frequently from the heart's blood.

The bacilli, as seen in smears from the lesions of an infected organ, are rod-shaped bacilli, about the size and shape of the *Bacillus mallei;* they stain readily with all the usual stains, but are not acid-fast, nor do they retain the stain when stained by Gram's method. Stained with Leishman's stain, they show a well-marked bipolar staining, the poles being stained a dark purple, while the bodies are blue.

Growth upon all the usual culture media is rapid and luxuriant, and occurs under both aërobic and anaërobic conditions, though more luxuriantly under the former.

For the first twenty-four hours the growth in broth is not very luxuriant, although as early as the end of the tenth hour of incubation it can be seen as a diffuse, faint haze; after twenty-four hours there is a general turbidity of the broth and a pellicle begins to form at the surface; this pellicle gradually thickens, until at the end of the fourth or fifth day it is a tough, resistant, wrinkled skin.

Upon ordinary peptone agar the growth appears in from eight to ten hours as moist, translucent, slightly raised colonies; in forty-eight hours these colonies have become opaque and thick, and are of a cream colour. In agar cultures of over a month's growth the colonies are dry with the middle portions wrinkled, and their colour is brown with a tinge of pink.

Upon salted agar, the salt being 1½ to 2 per cent. in strength, growth is slow, and appears as a thin layer, rather like a thin coating of white paint. A smear preparation from a salted agar culture shows that the bacilli are growing in dense felted masses, composed of very long sinuous filaments. If the strength of salt is over 2 per cent., growth is so slow that, in order to secure this curious change in the appearance of the bacilli, it is advisable to inoculate a large number of these, sufficient to form immediately after inoculation a thick, visible streak.

Upon glycerine agar containing 12 to 15 per cent. of glycerine the growth is rapid and luxuriant, but at the end of the second day the lowest third of the culture begins to acquire a wrinkled appearance, and this wrinkling rapidly extends to the whole growth, until after little more than a week's incubation the growth has become heaped up and rugose, not unlike a thick growth of tubercle bacilli.

Upon gelatine at a temperature of 18° to 22° C. growth is rather slow, but at the end of the third day of incubation there is a white streak of visible growth, and beneath this liquefaction of the gelatine slowly takes place. In stab cultures this growth produces very characteristic appearances; at the end of the third day there is a faint white streak of growth along the whole needle track, and at the surface the growth is spread out in the shape of a small white disc; by the fourth or fifth day the gelatine just below this surface disc is obviously liquefying; and by the end of the week, or a little later, the liquefaction has progressed to form a small cup of liquefied gelatine covered by a thick wrinkled pellicle; while along the rest of the inoculation stab is a white line of growth with extremely fine dots distributed in a radiating manner out into the surrounding clear gelatine.

Liquefaction of the gelatine has occurred in every case, but the actual rate of the liquefaction has varied considerably with the different strains of bacilli inoculated.

Upon potato a vigorous growth appears in twenty-four hours. As a rule at its first appearance the growth has been of a cream colour; but with bacilli from one or two cases the initial colour has been light yellow, not unlike that of young cultures of *Bacillus mallei*, but this yellow colour is very quickly lost. The potato round the growth is not discoloured, nor is there much spread of the growth away from the line of inoculation.

In litmus milk growth readily occurs. For the first three days there is no marked change in reaction, but later the casein is precipitated, a thick white fluffy sediment collects at the bottom of the tube, the whey becomes thick, and a violet-coloured scum dotted with white spots is formed on the surface.

The various sugared media have been inoculated with the bacilli, but, though growth is vigorous, there has never been the slightest gas formation in any of these media.

In young cultures upon all media the bacilli are actively motile, but this motility is almost entirely lost as the cultures age. In agar cultures the motility is often so diminished by the beginning of the third day that cultures examined then would be accepted as those of a non-motile organism. In broth cultures the bacilli retain their motility for a longer time, but after the tenth day of incubation they have become practically non-motile.

When present the motility is of a curious serpentine character.

We have no evidence of spore formation under any conditions, but our observations on this point are meagre.

The cultural and other characteristics upon which we rely for distinguishing this bacillus from other pathogenic bacteria are, I think, fairly obvious; those which we find of particular utility in the rapid identification of the bacillus are:

1. The rapid and luxuriant growth upon ordinary peptone agar.
2. The wrinkling which occurs so early in the growth upon glycerine agar.
3. The pellicle formation at the surface of broth cultures.
4. The appearances of gelatine stab cultures at the end of the third day, and after one week's growth.
5. The curious tangled masses of long filamentous bacilli found in cultures upon salted agar.
6. The active serpentine motility of the bacilli in young cultures, and its early disappearance as the cultures age.

When a rough preliminary examination of smears from diseased organs is being made, the bipolar nature of the staining with Leishman's stain is exceedingly useful in arousing an early suspicion of the nature of the infection.

For testing the pathogenic effects of the bacilli upon animals we have made use of guinea-pigs only.

Both subcutaneous and intraperitoneal inoculations into these animals have been made, and all strains of bacilli have been found markedly pathogenic. In all our experiments the animal has shown signs of very serious illness within a few days. At first the dosage of bacilli used was very large, 20 minims of a twenty-four hours' culture being injected as a dose, and in these cases the animals were either dead or moribund within forty-eight hours; but in the later experiments, made for diagnostic purposes, very much smaller doses have been given.

The results of the large inoculations have already been mentioned while describing the investigations into our first two recorded cases of the disease. The guinea-pigs die of a septicaemic disease, with well-marked local lesions depending upon the site of infection. With the large intraperitoneal inoculations the animals die so rapidly that the inflammation of the testicles, which occurs in the male pig, may not have time to develop to any very obvious extent; the swelling and redness may be so slight as to be easily overlooked; this no doubt happened in our first experiment. Therefore, if the occurrence of these characteristic local inflammations is to be utilized for diagnostic purposes, a very small dosage is requisite: we have found that half to one minim of an eighteen hours' broth culture, given either intraperitoneally or subcutaneously, is an ample and useful dose. This dose, given intraperitoneally to a male pig, causes a very well-marked and characteristic Strauss's reaction, which is obvious within thirty-six hours of the injection. Within the fourth or fifth day from the time of inoculation the guinea-pig is obviously seriously ill, and may die, even within five days' of injection; however, the exact dates of obvious serious illness and death have varied very considerably with the strains of bacilli used.

A small dose given subcutaneously gives rise to a rapid induration of the tissues around the site of injection, and this infiltration, or induration, is due to a matting of the tissues by a thick caseous exudate; in the caseous matter, which can be easily expressed, the bacilli are extremely

numerous, and can be readily demonstrated. This rapid local increase of bacilli after inoculation, and the obvious accompanying, inflammatory exudate into the tissues, are extremely useful in demonstrating the presence of bacilli, when they exist in very small numbers in any infective material.

The onset of serious illness is, of course, later with a subcutaneous than with an intraperitoneal infection, but in all cases serious and fatal illness has occurred, and, as a rule, has developed within seven days of infection.

Feeding experiments with contaminated food and drink have been carried out, and, so far as infection of the animals is concerned, have always given positive results. For purposes of clinical diagnosis, such experiments are too protracted to be serviceable, yet they have proved of great value as a convincing proof that the bacillus isolated from the lung and other organs of man is the cause of the illness; for by food infection we have succeeded in setting up in guinea-pigs a fatal septicaemic disease characterized by lesions of the lungs exactly similar to the human lung lesions. We have carried out four such experiments, and in all four cases have caused fatal illnesses; and have been able to demonstrate after death extensive lung consolidations due to this bacillary infection. These experiments will be referred to again when considering the method of infection in man.

In the first cases of the disease observed by us the lesions of the lungs were so obvious and striking that we were not unnaturally impelled to the view that we had to deal with a "lung disease," and that it was in the lungs alone that gross evidence of infection was to be expected. It is true that in the second case the recovery of bacilli from the spleen proved that, at any rate bacteriologically, the disease was more widespread; but at that time we failed to appreciate the importance of this observation. However, the isolation of bacilli from the heart's blood in Case III afforded further proof of the general nature of the infection, and in subsequent cases we found that the presence of bacilli could be generally demonstrated in the spleen as easily as in the macroscopic lesions of the lungs. We therefore adopted the view that the disease was essentially a "lung disease"; just as in typhoid fever though the most obvious lesions are confined to the intestine, the infection is general, so also in this disease, with its gross and obvious lesions restricted to the lungs, the local nature of the lesions gave no clue to the possible extent of the infection throughout the body.

These are the characteristics of the bacillus, as determined by us in Rangoon, and they have been found constant in all strains of the bacillus; but I would remind members of the Section that in Rangoon we have been working, not in a well-equipped laboratory with laboratory servants well trained to accurate methods, but that we have had to endeavour to do our best under somewhat adverse circumstances, and therefore while I have no doubts whatsoever as to the accuracy of our own results, there are, I think, many points in our bacteriological description which require more accurate investigation than we have been able to afford. Recently, by the kindness of Dr. Eyre, I have been permitted to repeat these cultural tests of ours in his laboratory. Upon the accurately standardized media used there, I have found that the growth upon glycerine agar, containing the high percentage of glycerine described by us, is very feeble, and does not accord with our description. From the few experiments I made there it appeared to be likely that growths similar to those obtained by us in Rangoon would be obtained on glycerine agar containing about 8 per cent. of glycerine. Personally I do not doubt that it is only a matter of short experiment to determine the exact percentage of glycerine necessary, but it is certainly important to note that up to the present I have failed to demonstrate in London the characteristics of growth upon glycerine agar which we have described from Rangoon. Upon gelatine also, as prepared in Dr. Eyre's laboratory, I have found growth exceedingly slow, so much so that although on using a very thick inoculation upon a gelatine slope I obtained liquefaction of the gelatine, yet the ordinary inoculation of a few bacilli failed to give liquefaction with the very slow growth which occurred, and the characteristic gelatine growths obtained by us were not obtained in England. However, here again I do not doubt the essential accuracy of our Rangoon observations, but we have been working with gelatine nothing like as firmly solid as Dr. Eyre's media, and also I do not suppose that our temperature observations should be regarded as accurate, for we have had to depend upon an ice chest as our cool incubator.

In other respects, the behaviour of the bacillus in Dr. Eyre's laboratory was, I think, similar to that in Rangoon. Dr. Eyre, moreover, was kind enough to confirm our observations as to the lesions caused by the inoculation of a small dose of the bacillus intraperitoneally into a male guinea-pig.

I had hoped to have had a series of tubes of dead cultures on view in the museum showing all the cultural characteristics described, but unfortunately the parcel seems to have miscarried in the post, and has not yet come to hand.

Such is the bacteriological description of this Rangoon disease. I am very well aware that the description leaves very much to be desired, addressed as it is to a company of expert bacteriologists; but I hope that it has not been uninteresting, in one respect at least, as an example of the great and growing difference between our present-day methods of working and acquiring medical knowledge and those not long ago in vogue.

DISCUSSION.

Dr. EYRE (London) confirmed the views expressed by Captain Whitmore in regard to the bacillus and the lesions caused by it. He was inclined to the opinion that the differences in cultural behaviour in the Rangoon experiments and those conducted in his laboratory were chiefly due to differences in the culture media. The lesions produced by the bacillus were so like those produced by glanders that in showing the characteristics of Strauss's reaction in the guinea-pig, for teaching purposes, he found that it was immaterial whether he selected the guinea-pig infected with true glanders or one which had been inoculated with the Rangoon bacillus.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

OPERATION FOR EARLY MAMMARY SCIRRHUS.

THE following case of mammary scirrhus must, I venture to think, be the earliest on which an operation has ever been carried out.

The patient is a lady aged 46 years. Seventeen years ago she had a small tumour removed from the left breast, which proved on examination to be a small cystic adenoma. This I know to be correct, as she had kept the tumour in spirit, and I have had the opportunity of verifying the statement.

In May, 1911, she consulted me about a small lump in the skin, situated above the superior internal quadrant of the right breast, about 2 in. below the right clavicle, and 2 in. external to the border of the sternum. This I removed under local anaesthesia, and was satisfied that it was a simple sebaceous cyst of the skin; the wound was sutured with a single horsehair stitch, which was removed after eight days.

In August of the present year she again consulted me, when three minute tumours were palpable in the situation, but not in the substance, of the scar of the operation last referred to. These I also removed under local anaesthesia.

The tissue excised was examined by Mr. S. G. Shattock, who reported that the chief nodule was no larger than a pea (7 mm. in diameter), and that on section it exhibited to the naked eye the macroscopic characters of a mammary scirrhus, which was confirmed on microscopic examination, the histological structure being perfectly typical of that disease.

Without delay I thereupon removed the whole breast, together with the pectoral muscles, and cleared out the axilla.

Mr. Shattock reported that there was no carcinomatous disease in any part of the breast, or in the structures removed, and made the following comments:

He had never heard of a primary mammary scirrhus of so small a size having been detected and removed; one reason for its early detection was doubtless the fact that it arose in the very periphery and thinnest part of the breast, and that the presence and particular situation of the previous scar must have directed the frequent attention of the patient to the spot itself. Its origin beneath the scar was not a coincidence; nor was it a reason either for supposing that the lesion previously removed was other than a sebaceous cyst. The sequence was best interpreted as indicating that the breast was in a particularly vulnerable condition—one, that is, in which a cancerous growth might have been incited by a very small degree of injury, the immediate cause of the growth being some trivial damage sustained by the outlying piece of mammary tissue at the antecedent operation.

Under these circumstances the subsequent removal of the whole breast, as a prophylactic measure, was quite justifiable, although the rest of the gland contained no carcinomatous focus.

Wimbledon. J. Edwin Bates, M.B., B.C.Cantab.

DELAYED CHLOROFORM POISONING ASSOCIATED WITH SEPSIS.

A girl aged 12 years, active, wiry, and with a good constitution, suffered from pain in the abdomen and vomiting, and had a temperature of 101° F. Symptoms being regarded as due to eating some strawberries, a dose of castor oil was given. Next morning the bowels acted, and the temperature was normal; however, some pain was still present in the lower part of the abdomen, but she was able to get up and lie about on a couch. The same evening, as the pain returned and she was sick again, I was sent for. The abdomen was flat, held rather stiff, but no more rigid over one part than another; there was no local tenderness, but pressure in the right hypochondrium caused pain referred to the right iliac fossa. The pulse was 108 and the temperature 99.6°. Next morning the temperature was 102.4° F. and the pulse 130; she had had one or two attacks of pain in the night.

Operation was advised and performed within forty-eight hours of the first symptom and within twenty-four hours of my first seeing her. The appendix was found to be perforated and gangrenous as to its last inch and a half; it was hanging free over the pelvic brim with half a pint or more of clear non-odorous fluid in the peritoneal cavity; the appendix itself had practically no mesentery, and had to be shelled out of its peritoneal coat; the peritoneum covering the caecum and adjoining part of the ileum was very much inflamed. The operation took fifty minutes, and a large drainage tube was passed down to the pelvic floor with a gauze wick inside it. The patient was put into a sitting position, and salines given per rectum, half a pint every two hours, and well retained; the anaesthetic used was chloroform, and she was some time coming round from it, but she did not require much, and the last ten minutes of the operation she had none; the exact amount used was unfortunately not known, but she was anaesthetized by my partner, who has a large experience in anaesthetics.

After the operation her pulse was 100, a fall of 20 as compared with before the operation. She passed a good night with several hours' sleep, no pain or vomiting. The wound was dressed, and considerable serous discharge found on the dressings. Thirty-six hours after the operation she was first noticed to be jaundiced: she was given castor oil by the mouth, which acted in three hours—a clay-coloured motion floating in thin dark fluid, containing some altered blood. Only a little serous discharge was found on the dressings. She was given some glucose by mouth, but vomited it, and it was then given by the rectum. The urine was high coloured, and contained a good deal of albumen, but there was no smell of acetone; the abdomen was soft all over, and was not tender; both temperature and pulse were about 100.

Sixty hours after the operation she became delirious and noisy, and eventually maniacal; there was very great thirst, and large quantities of water and glucose were administered by the mouth at this stage. The temperature was 91 and the pulse 104°. A fleeting erythematous rash was noticed on the flanks, the spleen was enlarged and the bowels opened several times with calomel. The urine passed was in good quantity and high coloured. Stupor, coma, and convulsions supervened, and death occurred eighty hours after operation, with temperature 109.4° F. The cerebro-spinal fluid was clear and not under pressure. Several injections of antistreptococcic serum were given, and also a stock vaccine.

No *post-mortem* examination was made, but the case seems clearly to be a combination of chloroform poisoning and sepsis. She could probably have survived either separately, but it is the combination that is fatal in these cases. The case should be compared with the one reported on p. 794 of the British Medical Journal for September 28th, 1912, and emphasizes the fact that ether, and not chloroform, should be used as the anaesthetic in these cases.

W. A. Rees.

A METHOD OF GIVING NITROUS OXIDE WITH THE ADDITION OF ETHER IN MINOR SURGERY.

Having witnessed the very painful recovery after nitrous oxide administered for the opening up of a whitlow, and having once had a somewhat similar experience myself, it occurred to me that the following method of giving nitrous oxide and ether has not received the attention it deserves.

The mention of ether is sufficient to call up a vision of violent emesis; administered, however, as an addition to nitrous oxide in the following manner, it will be found to be the exception for the patient to be sick, and not the rule.

The nitrous oxide anaesthesia is conducted in the usual way (an ordinary Clover bag being attached to the gas apparatus), but after the inhalation of the first bag of gas, which is, of course, refilled, the ether is turned on fairly quickly until the indicator stands at full ether. On the commencement of stertor the anaesthetic is withheld. This will give an anaesthesia about twice the length of N_2O alone. If a longer anaesthesia than this is required air is given from time to time. No to-and-fro breathing takes place at any time during the administration.

The difference to the feelings of the patient on recovery from a short though painful operation after this anaesthesia and N_2O alone has to be experienced to be fully appreciated.

J. K. Pedley,
Tunbridge Wells. Dental Surgeon.

COMPLETE INVERSION OF THE UTERUS.

On July 29th last I attended Mrs. P. in her second labour. She is about 25 years of age, of slight build and nervous temperament. Chloroform was given towards the end of the first stage, and on account of some delay in the second the forceps were applied and delivery effected quite easily. There being delay in the expulsion of the placenta ergot was given and steady pressure made over the uterus for about a quarter of an hour, but no traction made on the cord. At this stage the patient suddenly complained of sharp pain in the abdomen and the placenta was expelled from the vagina with a firm mass of tissue adhering to it which on examination was found to be a completely inverted uterus with the placenta still attached. This was peeled off and the uterus replaced into the vagina and steady pressure with the hand made for about ten minutes, by which time the uterus was restored to its normal position. More ergot was then given and firm pressure applied to the uterus through the abdominal wall for about half an hour. The organ was then felt well contracted in its normal situation. There was only moderate haemorrhage and very little shock. For a few days there was a slight rise of temperature, but the patient made an excellent recovery, and a vaginal examination a month after showed everything quite normal. In an extensive midwifery practice of over thirty years I have never before met with a case of inversion, and as it is stated in books to be very rare, once in about 180,000 cases, I am giving a brief history of the case thinking it may be of some interest to my fellow general practitioners.

Wealdstone, Middlesex. George H. Butler.

British Medical Association.

CLINICAL AND SCIENTIFIC PROCEEDINGS.

SOUTH MIDLAND BRANCH,

AT a meeting of this Branch on October 10th, Dr. WICKHAM spoke of a peculiar case of *Arthritis of the knee*, apparently following slight injury, in which various forms of treatment had been tried over many months without success. During the whole of the time the patient had slight sycosis of the chin, and the use of vaccines prepared from this eventually led to a complete cure. Dr. ROBSON read notes of a case of sudden onset of *Complete paralysis of both arms and legs*, seven days after a head injury. There was also complete anaesthesia of the limbs, but no bladder or rectum trouble. The plantar reflex was flexor. Dr. Robson showed how he arrived at the conclusion that the whole trouble was functional, after having discussed the various possibilities. This proved to be correct, the man being shown at the meeting, having recovered completely in the course of ten days. Dr. HICHENS and Dr. HAIGH read a short paper on *Familial jaundice*, and exhibited one member of the affected family. The cases were characterized by a partial jaundice almost constantly present, an enlargement of the spleen, and a peculiar heart murmur, which was quite different from a haemic murmur. Dr. Hichens considered that the jaundice was haematogenous, and was due to increased friability of the red blood corpuscles. Mr. ODGERS showed 5 cases illustrating the *Extended operation for mammary carcinoma* introduced by Sampson Handley. The cases had been operated upon from five to three years ago.

Reports of Societies.

MEDICAL SOCIETY OF LONDON.

Monday, October 28th, 1912.

Sir W. WATSON CHEYNE, President, in the Chair.

Hedonal Anaesthesia.

MR. C. M. PAGE, in a further communication on hedonal anaesthesia, said he injected the drug into the saphenous vein at the internal malleolus at a temperature of 105° to 110° F. He had had no fatal case attributable to the anaesthetic, and considered that those reported were due to overdosage. Safe anaesthesia was usually associated with a skin reflex. The cases unsuitable were (1) aged subjects and especially those with marked arterio-sclerosis, (2) cases in which there was active pulmonary disease, (3) cases in which the heart was not sound. The drug had been exceptionally successful in (1) cases of goitre including 5 of Graves's disease, (2) cases of shock associated with haemorrhage, (3) cerebral and spinal cases, (4) cases in which there was considerable post-operative pain. The method for properly chosen cases had a definite value, but was not free from danger, the latter being chiefly due to the ease with which an overdose could be given.

Dr. Z. MENNELL, in a paper on hedonal infusion anaesthesia, with special reference to its uses in intracranial surgery, said this method had been used in 310 cases at St. Thomas's Hospital since Mr. Page published his 200 cases. No death had occurred within twelve hours. The saphenous vein was chosen (1) because it is out of the way, (2) as a convenience to the anaesthetist, enabling him to take the blood pressure in the arm, (3) the vein is large and easily exposed, (4) there is no need for special apparatus as suggested by Moynihan. It is especially important to note the respiration, skin reflex, blood pressure, and the pulse. The following were contra-indications to its use: (1) Any operation about the air passages which caused haemorrhage into them, without preliminary laryngotomy, (2) cases of high blood pressure, (3) cases in which anaesthesia could be obtained as safely and as satisfactorily by other means. Hedonal was indicated in (1) operations in the neck—goitre, including Graves's disease, (2) ventral hernia, (3) intracranial operations. In the last it had special advantages—the anaesthetist was out of the way, any posture of the patient was possible as long as there was a free airway, there was no initial fall of blood pressure; it was possible to proceed further with the operation, thus avoiding a second stage; there was no vomiting afterwards, the prolonged sleep afterwards was invaluable, saline infusion could be given at once without further incision.

Mr. PERCY SARGENT said that hedonal had been used in 43 intracranial operations done by him. No deaths had occurred due to the anaesthetic. The operative fatality within the first forty-eight hours, possibly partly due to the anaesthetic, was as follows: In 60 chloroform had been given, with mortality of 20 per cent.; in 29 ether, mortality 13 per cent.; in 31 hedonal, mortality 3 per cent.

Mr. BARRINGTON WARD, in a paper on hedonal anaesthesia, its use in children, said he had employed it in 70 cases, with one fatality, in which lymphatism had been found after death. In only one other had he experienced any anxiety. Hedonal could not be extolled as a safe anaesthetic. It had certain advantages. It was a pleasant anaesthetic for the patient, and free from discomfort. For the operator excellent muscular relaxation was obtained, and the anaesthetist was out of the way. In acute empyemata in children, with hedonal, respiration was quiet, and no explosive coughing occurred. The after-results were excellent, sleep being procured for five to seven hours, and vomiting and acetonuria being absent or never serious. There were not a few disadvantages. Its immediate effect was probably as dangerous as chloroform; the estimation of the depth of anaesthesia was difficult; the slowness with which it was excreted demanded very careful administration, as after over-dosage resuscitation was almost a forlorn hope. The vein might be difficult to find and fit with a cannula. It was contraindicated in short operations, and in the usual list of operations, because of the time consumed. The indications for its use were therefore few. It was especially valuable in brain operations, in cases where the lung condition contra-indicated inhalation anaesthesia, and where the child dreaded another anaesthetic.

Mr. J. F. DOBSON quoted records from the Leeds General Infirmary, where 436 cases had been anaesthetized by hedonal. Of these, 44 had died (10 per cent.). During the same time the total operations had been 2,302, with 164 deaths (7.1 per cent.); 1,856 operations were performed by the old methods of anaesthesia, with 120 deaths (6.4 per cent.). He thought that most to be feared were pulmonary oedema, pneumonia, pulmonary thrombosis, infarction of the lung, and shock. The figures showed the frequency of pulmonary complications after hedonal. It was not suggested that in all the cases quoted the death of the patient was due to the anaesthetic; in all it was, in his opinion, a factor, and in many the chief.

ROYAL ACADEMY OF MEDICINE IN IRELAND.

SECTION OF SURGERY.

Friday, October 25th, 1912.

R. D. PUREFOY, P.R.C.S., President, in the Chair.

Subtentorial Decompression.

MR. A. A. MCCONNELL showed a case of cerebellar tumour for which Cushing's subtentorial decompression operation had been performed. Before operation the patient had headache, vomiting, partial optic atrophy, vertigo, and ataxia. She was also subject to seizures, in which all the muscles were thrown into a state of tonic contraction. The localizing symptoms were: Tinnitus in left ear; diadocokinesia, which was well pronounced on the left side; and constant deviation to the left side on walking. Optic atrophy was also more marked on the left side. Wassermann's reaction was negative. At the operation a tumour was palpated in the left cerebellar hemisphere. There was a free flow of cerebro-spinal fluid from the eighth to the tenth day, when the sinus closed. There had been no recurrence of headache, vomiting, vertigo, or tonic muscular spasms since operation. Hedonal was the anaesthetic used, the patient being in the prone position.

Hedonal.

MR. H. DE L. CRAWFORD reported 30 cases of hedonal anaesthesia. In all full surgical anaesthesia had been

obtained, and no death had occurred that was in any way attributable to the drug or the method by which it was administered. The following complications had developed after operation: One case of slight bronchopneumonia and one of acute oedema. The latter condition occurred in a boy of 17 years, who had been operated on for inguinal hernia. The dose injected was 1,250 c.cm., and the period of deep sleep lasted eighteen hours. The symptoms —fast pulse and fast respiration—subsided on propping the patient up and clearing the air passages. The cases included eight operations for cancer, two for brain compression and eight laparotomies. The greatest advantages noticed with the anaesthetic were the slight amount of blood lost in head cases and the very marked abdominal relaxation. The anaesthetic was contraindicated in the young and vigorous, who were not going to be subjected to severe loss of blood, and who were not collapsed. In the aged cachectic, on the other hand, the anaesthetic had been found most valuable. In the discussion which ensued, Mr. W. I. DE C. WHEELER mentioned that he had been using omnopon in simple operations, and so far had found it satisfactory. When given before the administration of ether patients when anaesthetized would remain so for hours. There was a complete absence in every case of anything in the nature of cyanosis. The procedure was very simple and the contraindications were practically *nil*. Dr. KIRKPATRICK, in referring to the point that the anaesthetist could administer hedonal in such fashion that he could be out of the way of the operator, said it should not be overlooked that, no matter how the anaesthetic was given, the anaesthetist must have access to the patient's head. Mr. CRAWFORD, in his reply, said that with regard to the danger of cyanosis if the anaesthetist remained at the patient's feet instead of at the head, he found it necessary to stay at the patient's head, and to signal when the tap was to be turned off. He considered the anaesthetic as safe as any other in practice.

ROYAL SOCIETY OF MEDICINE.

SECTION OF ANAESTHETICS.

Friday, November 1st, 1912.

Dr. J. BLUMFELD, President, in the Chair.

Death after Hedonal Anaesthesia.

DR. G. A. H. BARTON, in recording a death after hedonal anaesthesia, said that the virtues of a new anaesthetic were apt to be overestimated. One heard of the successes, but little of the deaths; he had only found three, including the present case, reported in the journals up to date; but these were excessive, considering the small number of administrations. Rumours of unreported fatal cases, however, were rife, and it was in the hopes of eliciting some definite information on the safety of hedonal, and as to the class of case in which it was contraindicated, that he reported this fatality.

The patient was an ill-nourished tailor, who had lately been on strike. There was no complaint of cough, and his respirations and temperature were normal. Nothing was noted amiss with his lungs. The pulse was, of course, rather poor. The operation was opening and draining both frontal sinuses, the posterior nares being first plugged with a sponge. The induction was very quiet in three and a half minutes, and after the injection of 400 c.cm. he appeared to be under. The rate of flow was slowed down, and at the end of five minutes the patient had received nearly 500 c.cm., and operation was commenced. The rate was now cut down to the slowest possible, and remained at that.

The anaesthesia was apparently one of the quietest and best the speaker had ever obtained. The only adverse point about it being that the patient's colour was dusky throughout. This the speaker attributed at the time to the post-nasal plug and to a towel over the mouth causing some obstruction. The operation lasted one and a half hours; 1,000 c.cm. were infused in all. A quarter of an hour before the termination of the operation the infusion was stopped and oxygen administered; this speedily improved the colour. No blood got into the air spaces during the operation, and the post-nasal plug was removed at its termination. The patient when the speaker left was still unconscious, but apparently in good condition. He died two hours after without regaining consciousness.

Dr. TREVOR read an account of the *post-mortem* findings, and said he attributed death to heart failure while the patient was suffering from the toxaemia of tuberculosis, such death having been hastened by the administration of hedonal and by interference with respiration through bleeding into the trachea. The man was anything but a fit subject for an anaesthetic, and his privations owing to the strike had reduced him very much. The blood in the trachea was not frothy, and probably some of it was of *post-mortem* origin. There was a mottled or "tigroid" condition of lung.

Dr. Z. MENNELL said he had given hedonal in 56 cases of cerebral surgery and not one death occurred within twelve hours of the operation. He had given it 198 times altogether, and at St. Thomas's some five hundred administrations of the drug had been given. It was contraindicated (1) in operations which would cause bleeding into the larynx or trachea, (2) patients with high blood pressure, (3) patients in whom anaesthesia could be obtained as safely and satisfactorily by other means. For cerebral surgery the drug was almost ideal. The prolonged sleep after the operation was often most useful and obviated the necessity of giving a narcotic. The rate at which he now gave the drug was 100 c.cm. every two or three minutes. Rapid administration produced cyanosis, which he regarded as the greatest danger signal in connexion with the drug. The corneal reflex was valueless; the skin reflex on the sole—causing toe movement and drawing up of the leg—was much more useful. One of his early cases died within eight hours of operation (enucleation of a cancerous tongue in an old man), and *post-mortem* a clot 4 in. long was removed from the larynx. By giving ¼ grain of morphine at intervals one patient was able to take as little as 240 c.cm.

Dr. SILK considered that the doses of hedonal recommended and used were too large. There seemed to be a difficulty in giving just sufficient to get the patient under while not giving too much. No doubt hedonal could be usefully employed as an adjunct to other anaesthetics. The advocates of hedonal now abandoned their recommendation of it as a universal anaesthetic, and acknowledged its limitations.

Mr. J. D. MORTIMER reminded those present that Dr. Barton published a short note on the case now detailed immediately after its occurrence, and he thought this should be done far more frequently when difficulties and disasters appeared during the trial of new methods. He knew, indeed, of two fatalities not mentioned that evening, and, so far as he knew, unreported, although they happened some months ago.

Dr. BARTON replied. He thought that if the reflexes were abolished hedonal became dangerous, and if the skin reflex was allowed to persist that the anaesthesia would be unsatisfactory.

Intratracheal Insufflation of Ether.

Dr. SILK explained the working of Dr. Elsberg's apparatus. He had had difficulty in introducing the catheter, but when once in position the anaesthesia was good. Mr. G. E. GASK and Mr. H. E. G. BOYLE showed their apparatus. Mr, Gask said that the apparatus was mainly intended for intrathoracic operations, and the anaesthesia was quite satisfactory. Mr. BOYLE explained the working of the apparatus, but, as he had only done 20 cases, he was not prepared to discuss the merits or otherwise of the method. Dr. SHIPWAY showed and explained the working of an apparatus made after the pattern of Kelly's.

SHEFFIELD MEDICO-CHIRURGICAL SOCIETY.

AT a meeting on October 24th, Dr. STANLEY RISELEY, President, in the chair, Mr. G. H. POOLEY described a series of cases illustrative of the *Importance of ocular symptoms in general disease*. They included instances of orbital cellulitis being the first observed effect of osteomyelitis, a case in which a localized redness of the eye had led to the discovery of diabetes, a squint which led to a diagnosis of pachymeningitis, and another muscle error which was for long the only warning of the growth of a cerebral tumour. There were also some cases in which embolism of retinal arteries occurred in apparently healthy young women who subsequently developed mitral disease. There were many patients who came for relief from headache by

spectacles, but were found to be suffering from renal trouble or tumour. The paper was illustrated by lantern slides. The following were among the cases and exhibits of other members:—Mr. E. F. FINCH: Two boys, aged respectively 8 and 10 years, who had both had a fall three and five months previously. One was a case of *Coxa vara;* the patient had never been laid up, and the mother had brought him merely on account of his limping. The other case was one of *Tuberculosis of the hip*, the first evidence of which had also been limping and pain in the knee. Skiagrams taken by Dr. RUPERT HALLAM showed separation of the head of the femur in the case of coxa vara, while another series illustrated the different conditions in the region of the hip-joint. Dr. ARTHUR HALL: (1) A case of *Intention tremor* of about five years' duration in an elderly man. No family history of tremors or other nervous disease. The tremor was quite distinct from the simple senile variety, and in certain respects simulated paralysis agitans. The condition did not seem to be getting markedly worse. (2) A case of *Intrathoracic tumour* in a little boy suffering from diabetes insipidus. The tumour was probably a mass of tuberculous glands at the root of the right lung.

GLASGOW MEDICO-CHIRURGICAL SOCIETY.

AT a meeting on October 25th, Mr. FREELAND FERGUS, President, in the chair, Dr. ALBERT A. GRAY gave a lantern demonstration on oto-sclerosis. This condition, he said, used to be called dry catarrh of the middle ear, but the more modern work of German observers had shown that the middle ear was not actually affected, and that the condition was due to an abnormal growth of the bone surrounding the oval window, whereby fixation of the footplate of the stirrup was brought about. A preparation from the temporal bone of a woman who had suffered from osto-sclerosis for eighteen months before death (this being the earliest case yet recorded from which specimens have been obtained), showed a ring of diseased bone round the oval window. Microscopic sections of the part showed that the change in the bone was one of rarefication, the bone having become spongy and containing large spaces filled with marrow. The diseased bone stained deeply, and thus was seen to be sharply delimited from the normal bone which formed the capsule of the labyrinth. Nothing abnormal was observable in the organ of Corti. A preparation from a more advanced case showed similar changes in the bone, with, in addition, extension of the disease to the footplate of the stirrup. The sharply defined limitation of the disease negatived the view that the process was inflammatory; the appearance presented was rather that of new growth. On the same ground he would exclude infection from the middle ear as a cause. The disease had been attributed to changes in the blood supply, and was certainly common in association with such conditions as arterio-sclerosis and syphilis, but often occurred independently of them. Probably the view of Manasseh of Strassburg, that oto-sclerosis was a nerve disease, was correct; the lack, however, of sufficient fineness in existing methods of research prevented the recognition of the nerve lesions in early cases. In support of this view were the facts that tinnitus was a very common early symptom, that the disease was nearly always bilateral, and that the conditions which aggravated it were chiefly such as exhausted the nervous system. As illustrating heredity in oto-sclerosis, the exhibitor showed genealogical trees of a number of families, many members of which were oto-sclerotic. In certain of these families the phenomenon of antedating was apparent, and in others was seen the very marked effect produced in a later generation by the union of two strains in which the tendency to oto-sclerosis was only slight. Certain of the family histories suggested also the possibility of an association between oto-sclerosis and deaf-mutism.

THE President of the Roentgen Society and Lady Mackenzie Davidson held a reception at Prince's Galleries, Piccadilly, on Tuesday evening last, to meet the Council and members of the Society. The guests had an opportunity of seeing the pictures in the gallery, and were afterwards entertained at supper.

Reviews.

THE CAUSE OF SHORT SIGHT.

DR. LEVINSOHN'S little work upon short sight[1] is a well-written account of his views and experiments. It commences with a critical review of the solutions of the problem which have been brought forward, and shows that not one of them adequately explains the pathological process. The book is beautifully printed in clear type upon light paper of a matt texture, giving no reflection of light. The German is easy and straightforward, there is a commendable absence of that rhetorical style which makes some German scientific works so difficult to read, nor are there any contractions which are sometimes even more puzzling to the English translator.

The author takes it for granted that the large mass of statistics now at our disposal show definitely that close study induces myopia, or at any rate causes it to increase in severity. All modern theories are founded upon this fundamental hypothesis. For many years the German authorities have paid great attention to the myopia problem, which is a very serious one in that country, where 50 per cent. of the educated classes are short-sighted. The schools have been well lighted and ventilated, proper desks have been fitted; in fact, nothing that medical science could suggest has been left undone. With what result? Frankly, with practically none! Hermann Cohn, the great advocate of school hygiene in ophthalmic matters, examined a number of medical students in 1866, and again in 1908. In each case he found the appalling percentage of 60, with an average degree of three dioptres. That is to say, that more than half of all German doctors have fairly advanced myopia! Levinsohn points out that if several years of careful prophylactic measures have had no influence upon the incidence of myopia, the hypothesis upon which these measures are founded must be faulty; the measures adopted are, therefore, in some way defective in scope or nature.

It has been suggested that, as near work causes myopia, the acts of accommodation or convergence, or both, must be the responsible factors. It has been assumed that these muscular movements raise the intraocular tension, and so cause a gradual lengthening of the bulb. Stilling singles out the superior oblique for especial blame. The researches of Hess and Heine have, however, conclusively proved that the strongest spasm of accommodation is unable to raise intraocular tension in the slightest. It has also been shown that no action of the external muscles has the slightest influence upon intraocular tension. The same is true for position. If an animal be held head down, the retinal vessels show no congestion and tension remains normal. The changes, too, found in a myopic eye are totally different from those known to be the result of increased tension, whether seen in infantile glaucoma (buphthalmos) or in true glaucoma. We may take it as proved, therefore, that the lengthening of the posterior segment of the eye, as seen in myopia, is not due to increased intraocular tension. It is not caused by the action of any of the intrinsic or extrinsic muscles. The author's theory is simple: When the head is bowed over a book or in writing the eye falls slightly forward, and so pulls on the optic nerve and stretches the posterior pole of the eye. Levinsohn first attempts to show that none of the contents of the orbit, neither the muscles, the nerves, blood vessels, ligaments, or fasciae take up the weight of the eye; it all falls upon the optic nerve, which has not sufficient bend in it to allow the eye to fall forward without tension on the retina. This, says, Levinsohn, causes all the changes at the nerve head and the gradual stretching of the sclera. It has actually been shown by Birsch-Hirchfeld that when the head is held horizontally with the face downwards the eyes come forward 1.7 mm. The author has confirmed these results by the use of a capsule which he placed on the eye and connected with a Marey's drum, and obtained a graphic record. He considers that, as muscular tissue is very elastic, it cannot hold up the eye; in fact, were it to depend upon the

[1] *Die Entstehung der Kurzsichtigkeit.* Von Dr. Georg Levinsohn. Berlin: S. Karger. 1912. (Roy. 8vo, pp. 88; Abb. 3. 2s. 6d. or M. 2.50.)

muscles, the proptoses would be 5 mm. in the horizontal position. The author has hung young cats, rabbits, and dogs in bags head downwards for several hours a day for periods as long as ninety days, and in 12 cases he says he has increased the refraction in the myopic sense. The increases range from 1 to 4.5 dioptres. In no case did he find any anatomical changes at the nerve head.

It would appear that the author's destructive criticism of the older myopia theories must be accepted, since none of them account for the facts. But we must at once say that we hold the same view about his own theory. Levinsohn has evidently forgotten that the muscles of the eye are not mere elastic bands; they have a tonus, and when they are divided the eye becomes slightly proptosed, as he himself mentions. The refraction of the animals used was determined by retinoscopy and by the refraction ophthalmoscope, and by several independent observers. We notice that the refraction noted before the experiment varied from 1 D. to 3 D., which is not what one would expect. In any case retinoscopy under such adverse circumstances is not very reliable. It is difficult enough in a human baby to get an accurate estimation; in a kitten it must be immensely more so. We should like to have these experiments confirmed before accepting them with any confidence. But there are objections to the theory upon much wider grounds. The worst forms of myopia are more common among labourers than among the educated classes (Hertel, Otto and Ankele, Siebenlist, Jaspers)—a fact which lends no support to Levinsohn's theory. Again, it is quite common to find extreme myopia up to 20 D. in one eye, the other being normal. Why should the weight on the eye stretch it on one side and have no action upon the other? Again, high myopia in children under school age is far from uncommon, and many of these children have never been bent over any work. Myopia is far rarer in England among the cultured classes than in Germany; the figure would be nearer 10 per cent. than 50 per cent., and yet our young doctors bend over their work almost as long as the Germans do. The key to the whole problem may be afforded by the fact that since games have been introduced into the Swedish schools myopia has been remarkably less common. The close confinement of German boys, the long hours, and lack of exercise leads to loss of tone, which in many cases causes the sclera to become flabby and distensible; and as it stretches it becomes still more distensible, a vicious cycle being set up. The low forms of myopia are probably due to a slight overgrowth in length. The baby's eye is hyperopic, it gets less hyperopic, but the final stage may be hyperopia, emmetropia, or slight myopia.

CATHOLICISM AND SCIENCE.

It has been considered necessary to demonstrate the fact that men of light and leading in science were also devout Catholics, and found no difficulty in maintaining both positions simultaneously. With this object a book has been published, under the editorship of Sir Bertram Windle,[2] containing biographies of Thomas Linacre, Andreas Vesalius, Nicolaus Stensen, Aloisio Galvani, Laënnec, Johannes Müller, Sir Dominic Corrigan, Angelo Secchi, Johann Gregor Mendel, Louis Pasteur, Albert de Lapparent, and Thomas Dwight. It is interesting to note that of these twelve men seven were members of the medical profession—a fact which is in itself a refutation of the old saying, *Ubi tres medici duo athei*.

The lives are by various authors. Professor Pye, of University College, Galway, deals with Linacre, whom the Royal College of Physicians of London honours as its founder. Linacre was one of the early humanists, and though he has left little or no trace in science, he did much for its endowment at Oxford and in London. He was physician to Henry VIII, but abandoned the profession to enter the Church in 1510. He was the friend of More, Erasmus, Colet, and of all the leading humanists in his time. His writings are chiefly translations from Greek, made directly from manuscripts, into Latin. They include the works of Galen in many volumes. He was also the author of a series of grammars, and it has been suggested that Browning had him in mind when he wrote his "Grammarian's Funeral," apparently on no better ground than that "*Calculus* racked him."

Vesalius is treated by Professor J. J. Walsh, who shows once more that in Italy, under the very nose, if we may say so, of the Pope, dissection was carried on publicly and extensively. The difficulty met with by Vesalius in procuring material in his own country, and sometimes in Italy, was due not to religious feeling, but to the sentiment that up to the passing of the Anatomy Act in 1832, made "resurrection" the chief source of supply of dissecting-rooms in this country. Dr. Walsh takes pains to refute the inferences of Sir Michael Foster and others from some passages in Vesalius's great work that he was a sceptic. He was, we take it, one to whom Don Pedro's description of Benedick in *Much Ado About Nothing* might have been applied: "For the man doth fear God, howsoever it seems not in him by some large jests he will make." This was a type very common in the sixteenth century, and is illustrated by Rabelais and Erasmus, who, however freely they may have spoken, never openly severed themselves from the Church.

Stensen, whose life is written by Sir Bertram Windle, was a remarkable man and had a remarkable life. He was by birth a Dane, and as a student at Copenhagen discovered the duct of the parotid gland, which still bears his name. He was professor of anatomy at Padua, and afterwards at Copenhagen. He extended his scientific researches to geology, in which he is now recognized to have been a pioneer. During his stay in Italy he became a Catholic, and later he took priest's orders. He afterwards became a bishop in Germany, where his labours as an ecclesiastical administrator left him no time for the prosecution of his scientific work. Of Laënnec an appreciative account is given by Professor Collingwood, of University College, Dublin. His faith and piety were no less remarkable than his intellectual power. It would be superfluous here to give a summary of his work, which is so well known. An account of him was given in the Journal of July 6th, 1907. We may call the attention of the writer to the fact that Laënnec's Christian names were not "René Théodore" but "René Théophile Hyacinthe." Johannes Müller is dealt with by G. A. Boulenger, F.R.S. Born at Coblentz in 1801, he first thought of becoming a priest, but after consideration decided on medicine. Of his work as a biologist it would be superfluous to speak. He was the master at whose feet sat most of the physiologists of the first half of the nineteenth century. Though some of his writings show a somewhat materialistic tendency of thought, he never dissociated himself from the religion in which he was brought up. The life of Sir Dominic Corrigan is told by the late Sir Francis Cruise, who knew him well. He was an eminent physician who contributed something to medicine, but he seems rather out of place among men of science.

Thomas Dwight succeeded Oliver Wendell Holmes in the Chair of Anatomy at Harvard. His anatomical work had reference largely to variations in the human skeleton. He was President of the American Anatomical Association, and his election to that position shows the value placed on his scientific work by his colleagues. He was also a man of the deepest religious faith, and an active controversialist on behalf of his church when circumstances seem to call for it. Professor McWeeney, in his account of Pasteur, gives one of the clearest accounts of the scientific work of that great investigator we have ever seen. The Rev. G. A. Elrington gives a lucid summary of the work of Mendel. Here, as in the case of Stensen, one cannot help regretting that the scientific work of such a man was interrupted by the press of official business thrust upon him as Abbot of an Augustinian monastery. Professor William Bergin, of University College, Cork, gives a short account of Galvani and Volta, recognized pioneers in electrical science. Father A. L. Cortie, S.J., himself an astronomer of note, records the achievements of the famous Jesuit astronomer, Angelo Secchi, and Father John Gerard briefly relates the life of Lapparent, a prominent geologist, who in 1907 was appointed Permanent Secretary to the Académie des Sciences in succession to Berthelot.

The book is illustrated with portraits.

[2] *Twelve Catholic Men of Science.* Edited by Sir Bertram Windle, M.D., Sc.D., F.R.S. London: Catholic Truth Society. 1912. (Cr. 8vo, pp. 252.)

CAUSE AND TREATMENT OF GALL STONES.

THIS monograph on gall stones, by Dr. GRUBE and Dr. GRAFF,[3] is most appropriately the joint work of a physician and a surgeon, for if the diagnosis, prophylaxis, and early treatment of the disease usually fall to the former, it is ultimately upon the latter we must generally rely to effect a permanent cure. In their preface the authors say that their object in writing is to afford practitioners a guide to the recognition and a counsellor in the treatment of this very common, most painful, and distressing disease, and in our opinion they have written a book which fulfils their aim, although perhaps it might be said that the recent valuable contributions on this subject by Professor Hans Kehr (BRITISH MEDICAL JOURNAL, July 28th, 1906, p. 199), Aschoff and Bacmeister (BRITISH MEDICAL JOURNAL, May 21st, 1910, p. 1237), and Ludwig Arnsperger in *Albu's Encyclopaedia* (BRITISH MEDICAL JOURNAL, May 18th, 1912, p. 1134) fairly occupy the ground. Of the books to which we have just referred, that of Aschoff and Bacmeister is the most important, since it would revolutionize the doctrine of the etiology of gall stones as taught by Naunyn. The authors of the present volume accept unreservedly the new teaching, which is that pure cholesterin stones may form in aseptic gall bladders, but that all other forms are the result of infection, and that the inflammation is not the consequence of the gall stones, but that these are the result of an inflammation set up by bacterial infection. They are convinced that the best operation is complete removal of the gall bladder with or without drainage of the bile passages, and they say that while the ideal of a mortality of 0 per cent. can, perhaps, never be attained because of the risk from pneumonia, embolism, collapse and anaesthetic accidents, yet the operation will have the most favourable results if it is not too long delayed. At the same time they would reserve surgical treatment for those cases which have not yielded to medical treatment or those in which the condition is undermining the health and happiness of the patient. They give the following indications for operating: (1) Chronic or recurrent pains which are so severe that the patient's capacity for work and enjoyment of life are interfered with; (2) all acute inflammatory processes which point to a severe infection, so that peritonitis, perforation, and cholangitis are to be feared; (3) all chronic inflammatory processes with septic symptoms; (4) chronic obstruction of the common bile duct; (5) cases which suggest the idea of cancer. They give the following as the conditions which an operation must fulfil: (1) It must remove the cause of the disease, especially the inflammation; (2) it must open up the passage of the bile ducts; (3) it must, as far as possible, prevent the new formation of gall stones. Dr. Grube is somewhat sceptical of the value of specific remedies for the solution or expulsion of gall stones, and relies more upon diet and the free use of alkaline mineral waters. In infection of the gall bladder following typhoid fever he urges the persevering use of urotropin, which is excreted by the bowel, and has the specific power of killing the typhoid bacilli.

THE UNDERWATER INTERNAL BATH.

DR. ANTON BROSCH, the author of a monograph on the "underwater internal bath,"[4] is of opinion that the tendency of modern therapeutics is to revert to the use of natural forces for the cure of disease. So far, internal water treatment has been chiefly confined to its use by drinking and enemata or irrigation, but lately both gynaecological examinations and massage under water have been introduced with success, and he is of opinion that there is a great future in store for the underwater method. Any increase of pressure inside the body causes discomfort or pain, but when this is compensated by increased external pressure the patient feels nothing, and large quantities of fluid penetrate high up into the bowel under low pressure, so that it may be used without risk of over-distending the bowel or tearing adhesions, and even in the presence of inflammatory adhesions, exudations, or peritonitis. He claims that his method is not only not disagreeable, but produces a marked sensation of well-being. It may be used with any kind of water, hot or cold, or hot and cold water may alternate. He recommends its use for all neurasthenic conditions, especially of sexual origin, for constipation, and even for cancer of the bowel. He believes it is more likely to conduce to the prolongation of life than Metchnikoff's sour milk treatment. The objects to be attained are: (1) The giving of an enteroklysm by permanent irrigation; (2) this must be carried out under water; (3) all risk of high pressure or of injury through the quality of the irrigation stream must be avoided; (4) the communication of infectious diseases must be prevented; (5) there must be no soiling of the external bath water; (6) insanitary contamination of the bath waste water must not be allowed. These conditions are not easily realized, but Dr. Brosch has invented an apparatus which he calls the "enterocleaner," a saddle-shaped pan upon which the patient sits in the bath; the pan communicates by one tube with an irrigation pail, and by another with a closed pail, which receives the contents of the bowel. The mechanism is so arranged that the patient sitting in the bath can regulate its action by himself. The author claims for this method the good effects of such a radical operation as that of Arbuthnot Lane, without any of the danger necessarily attending so grave a surgical operation.

MEDICO-LEGAL EXAMINATIONS.

THE small volume by Sir JOHN COLLIE on *Medico-Legal Examinations and the Workmen's Compensation Act*[5] (1906) will be useful to those medical men who have not had many opportunities of acquiring experience in the examination of claimants for compensation for damages. The author has gained his knowledge by being an examiner to a large corporation and to an insurance office, and the many entertaining examples from his routine work which he relates clearly display his sagacity. The book contains chapters on the detection of fraud, contracting out, a summary of the compensation laws, the intention of the Workmen's Compensation Act, and proposed amendments. There is neither table of contents nor index, and the book would be improved by the addition of these. On the subject of hernia the author is informing; he states that with a perfectly-fitting truss any one can do manual labour. If, however, he were an employer of labour he should never employ any one with a hernia, even though he wore a truss, for workmen are careless and the day arrives when the truss does not fit; strangulation may occur and the employer will be held liable. An announcement which ought to attract the attention of operating surgeons—for it is no doubt founded on personal observation—is that "one is loth to recommend operation for radical cure, as some 15 to 20 per cent. of attempted radical cures are failures." This is interesting, as some authorities say that the recurrences should be under 5 per cent. for inguinal hernias.

The author alleges that medical certificates of unfitness for duty are too easily procurable, and he holds that greater use should be made of medical referees, particularly at the arbitration, in view of the conflicting statements of partisan witnesses. Sir John Collie has many suggestions as to the amendment of the Workmen's Compensation Act, of which doubtless notice will be taken in the proper quarter when the question of an amending Act comes before the House of Commons. For example, he thinks there should be a right of appeal from the decision of a medical referee to a board of three medical referees. In Schedule 1, Section 15 of the Act, which sets out that the county court judge may refer a matter to a medical referee on application being made to the court by both parties, he thinks that the words "either party" should be substituted for both parties. We have the impression that the words stood so in the bill when it was introduced.

[3] *Die Gallensteinkrankheit und ihre Behandlung vom Standpunkt des inneren Mediziners und Chirurgen.* Von Dr. Karl Grube, Prof. in Bonn, und Dr. Henry Graff, Prof. und Dirigierender Arzte der Chirurgischen abteilung des Marienkrankenhauses in Bonn. Jena: Gustav Fischer. 1912. (Roy. 8vo, pp. 178; mit 3 lithogr. Tafeln und 20 Textfiguren. M.7.)

[4] *Das subaquale Innenbad.* Von Privatdozent Dr. Anton Brosch, unter mitwirkung von Dr. Otto Von Aufschnaiter. Zweite vermehrte Auflage. Leipzig und Wien: Franz Deuticke. 1912. (Roy. 8vo, pp. 118; Abb. 10. M.3; K.3.60.)

[5] *Medico-Legal Examinations and the Workmen's Compensation Act.* By Sir John Collie, M.D., J.P. London: Baillière, Tindall and Cox. 1912. (Demy 8vo, pp. 128. 5s. net.)

On the matter of the use of the medical assessor, the author is a strong advocate of freer facilities being given to either party to a suit to have a medical assessor sitting with the judge. The book will no doubt be serviceable as an elementary treatise on the subject with which it deals.

NOTES ON BOOKS.

In a leather-clad volume of vest-pocket size entitled *Prescribers' Formulary and Index of Pharmacy*[1] Mr. T. P. Beddoes of the London Skin Hospital has endeavoured to supply the information that a prescriber is likely to need and to obviate any uncertainty as to what drugs any ordinary pharmacist is likely to have in stock. The matter, which is well arranged, falls into four parts, internal, regional, and external treatment, and synthetic preparations, definite prescriptions of proved value in addition to doses of individual drugs being supplied. It is a favourable specimen of volumes of this order.

A sixth edition has been published of Miss Oxford's *Handbook of Nursing*.[2] The matter throughout has been carefully revised by, among others, certain members of the staff of Guy's Hospital. It is as an offspring of the training of that institution that the book may be regarded, since its foundations are more or less extensive notes of the lectures on nursing, surgery, and medicine, delivered to probationers thereat. Although the book is very readable as a whole, the information supplied is at times distinctly sketchy and not very systematically arranged.

[1] *Prescribers' Formulary and Index of Pharmacy.* By Thomas Pugh Beddoes, M.B., B.C.Camb., F.R.C.S.Eng. London: Baillière, Tindall, and Cox. 1912. (Pott 16mo, pp. 144. 2s. 6d. net.)

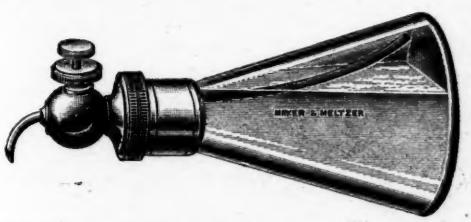
[2] *A Handbook of Nursing.* By M. N. Oxford, of Guy's Hospital. Sixth edition. London: Methuen and Co. 1912. (Cr. 8vo, pp. 327. 3s. 6d. net.)

MEDICAL AND SURGICAL APPLIANCES.

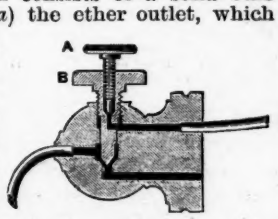
Continuous Drop Bottle for the Administration of Ether by the Open Method.

Mr. L. Ernest Acomb, M.R.C.S.Eng., L.R.C.P.Lond., D.P.H.Oxon. (Honorary Anaesthetist, Newport and County Hospital), has devised a drop bottle to meet the essentials of a good bottle, which are, he considers:

(*a*) That when not in use there should be no escape of ether; (*b*) that it should be able to be carried in a bag without leaking; (*c*) that it should give a continuous drop of the required size at equal intervals, these intervals being made longer or shorter at the will of the anaesthetist; and (*d*) that the bottle should be large enough to obviate frequent refilling, and should be of a convenient shape.



The bottle, of glass, is of a capacity of about seven ounces, is conical in shape, has a wide base, has no shoulder, so that the ether is entirely used up, and the ether surface always covers the outlet. A metal collar is firmly fitted to the neck of the bottle, over which the upper portion is screwed. The upper portion consists of a solid bulb having two channels in it, (*a*) the ether outlet, which projects beyond the bulb as a small tube, and (*b*) the air inlet. The amount of ether allowed to flow through the outlet is regulated by the screw B. This screw is so adjusted that a drop of any size may be obtained at desired intervals, and when screwed up it allows no ether to flow.



The air inlet is obtained from the flattened screw A, which pierces B and allows air to enter through the air inlet tube projecting to the bottom of the bottle. To use the bottle, the air inlet screw A should first be loosened, then the controlling screw B regulated. The bottle is made by Messrs. Mayer and Meltzer.

THE MOTOR SHOW.

FROM THE MEDICAL MAN'S POINT OF VIEW.

By H. Massac Buist.

The eleventh yearly International Motor Car Show promoted by the Society of Motor Manufacturers and Traders, and opened at Olympia, Kensington, on November 8th, under the patronage of the King, has a special message, or perhaps one might say series of messages, for the medical man, whose requirements may be considered to lie almost wholly along the line of products for those of moderate means. The first thing he must be sure of if he gets a car of any sort or size is reliability.

The sum of money at his disposal in the way of initial expenditure on the acquisition of a vehicle has often been such as to lead him in the wrong direction of goods that are merely cheap in nominal price without possessing those qualities of enduring service which alone can make them of value to the owner. Hitherto, let us bear in mind, the limited scale of output of the average manufacturer, combined with the tendency to divide it up between a wide range of models, has resulted in the impossibility of greatly reducing the price, while during the past twelvemonth, strikes and other troubles apart, the coming into force of the Insurance Act alone is adding to the cost of producing cars to an average extent of about £4 a chassis in the case of home-made goods. Further, in cars of high quality, no matter what the power, there is a fairly general tendency not to reduce but slightly to increase the cost. Analysis proves, however, that in such cases, more is always being offered for the increased cost either in the way of additional accessories, or of proportionately larger or more powerful engines, or in the standardizing of larger sizes of tyres, and so forth, so that in point of fact there is no case in which a 1913 car is dearer than the current types; on the contrary, in the majority of cases, if we compare this year's with next season's types, it is to find that the newer patterns provide much better value for the money.

The Outstanding Note of the Exhibition.

Possibly the ordinary visitor to the Motor Show could go to and come away from it without gleaning the least hint of what was of most significance in it, alike to the prospective purchaser and to the motor industry in Europe. He would see each maker's wares, including those of the Wolseley Company, which he would think very good value, and would merely wonder why the smallest 12-h.p. model was no longer marketed. The brief explanation is that this, our largest motor carriage manufacturing concern, owned by Vickers, and which hitherto has followed the general practice in providing a many-model programme, has abandoned that and is devoting the whole of its shops to the production of two models, being besides prepared to lay down, in batches of only fifty at a time, a very large six-cylinder type. When it is had in mind that the Wolseley shops at Birmingham can be measured by the half mile, it will be appreciated that for all practical purposes this, our largest firm, is concentrating on two models only. Moreover, the lesser of them is not the 12-h.p. but a 16–20-h.p., worm-driven, four-cylinder machine embodying various improvements on the 1912 pattern, and possessing, besides, better coachwork than has hitherto been fitted, as well as the necessary provision for the equipment of a self-starting apparatus; yet the price of the whole is only a few pounds more than the cost of the now abandoned (1912) 12-h.p. type. When the model offered for 1913 has been examined by men in the motor trade, not a few have spoken of it as being marketed at a cut price; but this is quite incorrect, for there is as good a profit on it as on any vehicle the Wolseley Company has yet put on the market. There has been no elimination of features from the design, and as much material and workmanship as ever goes to the production of the vehicle, while more expensive automatic machinery than before has been included in the factory equipment for the purpose of producing it, so that the sole explanation of this offering of so much better value for money is that, instead of turning out the type by a hundred or two at a time, a minimum of four thousand identical vehicles is to be produced at the factory at the end of twelve months; this means that the usually wasteful period of "turning over," as it is

called, is eliminated, and the works are to be kept going at full output for every week in the twelve months to the effecting of further economy.

A Fresh Way to give Better Value.

But it were quite erroneous to conclude that this is a copying of American methods. On the contrary, it is everything that is not done there, as anybody might gather who considers the size of the Wolseley shops, for they occupy about as much ground as so-called American motor car factories whence issue machines at the rate of tens of thousands a year. The explanation is that the American principle, particularly in cheap car production, would be more correctly described as assembling from parts produced by specialists than as manufacturing cars, whereas the British method consists in producing practically every part of a car at the works whose name it bears. Hence there is no possible comparison, nor any attempt on the part of the British industry to reproduce American scales of output. It has been very hard to convince the British motor manufacturer that if he had the pluck to try it he would find he had a market for a thousand cars where he imagined he had an opening only for a hundred; and it remains true that we have not markets for ten, twenty, thirty, forty, and even a hundred or more thousand cars of a single type in any given twelvemonth, which is the situation that obtains in America. Why none should fail to take note of the Wolseley development, therefore, is that it gives the European public the true advantage that should come to it from the competition between the European and American motor industries, and a solution of the problem, from a manufacturer's point of view, of how to keep the industry going in this country in legitimate fashion and without embarking on anything so disastrous and useless as a policy of imitation. We may take this Wolseley example as illustrating the all-important point of a vehicle designed in a particular country to suit the requirements of the conditions under which it is to be used. Moreover, it will be found during next and the following seasons that the move which this concern has just made will practically settle the future policy of other motor manufacturers in Europe. Already by far the majority of them are inclining to give up the many model policy which divides an output of a thousand or fifteen hundred cars between anything from four to eight models. All the firms that have made most distinctive hits lately are content with either three or two-model programmes, as instance under the former head the Sunbeam concern, which has revealed itself to be the producer of the outstanding car of the last twelve months in competition, and under the second heading the Crossley firm of Manchester, which has also come to the front in another way since the holding of the last motor show. Time was, of course, when manufacturers were feeling their way, and when the motor business was a much more personal affair than it is to-day; hence they found themselves tempted to try and produce cars of every conceivable size and power, so that there might never be an excuse for them to miss an order. To-day the error of that policy is beginning to be realized all round, for surely it is impossible that any one firm can expect to give the best value for money in the matter of every size and make of car. This is the age of specialization in motor cars as in other affairs, and the realization of the fact is what will make the motor season of 1913 a historic one; incidentally it will prove of greater service to the medical man than any it has been possible for the industry to render him in the past.

A Possible Objection Answered.

The medical man is unwise unless he treats his motoring as a strictly business proposition, hence he may be expected to raise a very practical objection against what we may call the fruits of this new era in car production. Thus he may say: "Good, I can manage to pay the extra few pounds to get a larger size of car and a more perfect vehicle than the type that was previously within the range of my purse; but it will be of little use to me because I cannot afford the extra cost of running it." It is safe to assume that if there is any section of the motoring community which, as a whole, uses its cars in a reasonable manner, it is the medical fraternity. The vehicles are needed for strictly business service, which is quite a different thing from scorching. Let us suppose that the medical man finds that a given 12-h.p. car will do all in the matter of speed that he needs, and that he is faced with the proposal to get a 16-h.p. for only a few pounds more. Being a quite up-to-date machine embodying the latest refinements, provided the owner is prepared to drive it in the same manner as the 12-h.p. machine in regard to accelerating the rate at which he climbs hills and the manner in which he applies the brakes, there need not be a five-pound note a year difference in the outgoing or running costs of using the larger and more powerful vehicle for doing precisely the same amount and kind of work, and, as such a machine will be able to do that work so very much more easily than a less powerful one, it follows that there would be a saving in the actual wear and tear bill in using the larger vehicle. And the owner has all the time the advantage of enjoying the use of a more luxurious machine, for a car never runs better than when it is working well below its maximum power. He has, besides, that extra body accommodation which is possible with the larger chassis. The cost, in other words, is the same if the conditions of use are reproduced exactly; but if the owner wants to use the larger powered car as near to the top of its performing as he would if using the lesser powered one, and wants to travel about the country at 30 per cent. greater speed and very possibly to put 25 to 40 per cent. on to mileage year in and year out, the costs will go up very appreciably. But that is a matter entirely optional with the owner.

The Age of the Self-starting Car.

These remarks have been necessary because we are arrived at the time when we must deal with quite a fresh phase of motoring, and one which furnishes not only the key to the meaning of the present exhibition, but which will enable the medical man to understand also the developments which will follow in its train next year and the year after. Compared with this, in fact, the other features of the show are of minor importance; yet, happily, some of them are of most direct interest to the profession. It may be recalled that last year I wrote in these pages of the coming of what may be styled the first positive self-starter in the combination Cadillac electrical device which started the engine, fired the mixture in the cylinders, and lighted the vehicle. Though stowed away in an insignificant corner of the building, that exhibit constituted the outstanding feature of the show, as may be judged from the fact that in the twelve months that have since elapsed this device has proved its worth in the hands of 12,000 users, as well as under a Royal Automobile Club test of an unprecedentedly severe character, in that it consisted first of choosing three cars at random from a consignment and of starting each of them up 1,000 times by the electrical energy stored aboard them, and without affording any opportunity, such as a run along the road, for those supplies to be replenished; yet there was not a single falter. Then to try the efficiency of the lighting and firing system the machine was taken over a period of a fortnight up and down the highways of the country with the full set of lights on all the time, including the whole of the luncheon intervals—a test to which no car electric lighting system has ever been submitted. It came out absolutely all right, the only thing that happened being that one or two bulbs blew out; they, of course, have nothing whatever to do with the plant. We see bulbs blow out on tube railways, as well as in our own homes, any day. Moreover, this device is now brought forward in a further improved form which reduces its weight, size, and number of parts. The single voltage system has been successfully adopted in place of the double variety, thereby eliminating the controlling switch, much wiring, and other parts. The voltage regulator governs the charging of the battery in automatic fashion, reducing to a minimum the attention needed on the part of the user.

Independence for the Medical Man.

The net result of this development as concerns the industry in general was that at the motor show held in America in the early months of this year nearly every manufacturer found it necessary to fit some so-called starting device, the vast majority of them proving to be utter failures, a circumstance probably to be accounted for in

large measure by the fact that they had not been sufficiently tested. Be that as it may, as regards motors marketed in Europe, the influence of the self-starter—which is of not a little importance to the medical man, since with the advent of detachable wheels, car maintenance contracting businesses, and so forth, it is now possible for him to dispense with the services of a motorman to accompany him on his rounds—is apparent at the present show in a variety of directions. Thus the Lanchester concern comes forward with an apparatus consisting of a dynamo mounted on a bracket projecting from the brake casing and chain-driven off the transmission shaft. The dynamo spindle is fitted with a pinion meshing with teeth cut on the flywheel. The pinion is slid in the gear with these teeth by means of a starting lever placed conveniently to the driver's left hand, the ignition of the engine being automatically retarded whenever this lever is worked, so that should it have been left advanced by any oversight no harm can be done. After starting up the motor the dynamo charges the cells and, when needed, serves to light the car. Another device is brought forward on the six-cylinder White petrol car; all that is necessary is for the driver to pull a lever on the dash, whereupon a dynamo that is driven by chain from the magneto shaft starts the engine, the batteries furnished being powerful enough to supply current sufficient for turning the motor over for about half an hour. As soon as the engine begins running under its own power the speed of the electrical starting motor is increased, whereupon it becomes automatically a generator for recharging the batteries up to the necessary amount. These batteries also supply the current necessary for lighting the vehicle. It will be noted, however, that both these devices are of a supplementary character, and do not fire the mixture in the cylinders.

In another direction there is a simple spring device introduced by the Star firm, and so designed that should it fail to start the motor before its energy is expended a crank can be put in in the ordinary way, and, by the employment of reduction gear, the spring can be wound up with no more force than a child can easily exert, whereupon it is ready to do its work again. This device does not turn the crank-shaft many times, but while it is working it turns it very fast—at 300 revolutions a minute—so that it is guaranteed that any engine in proper working order that can be started up on the magneto by hand would be easily started by this apparatus.

Variations of the Compressed-air System.

The Newton and Bennett self-starter of the compressed-air sort has long been a feature of the S.C.A.T. machines, on which it is standardized, as is another air-compressor device on the Adams cars; but new apparatus of this sort is brought forward this year by the Wolseley Company, which combines the device with an automatic tyre inflator. The minimum pressure which will start the Wolseley motor when warm is about 50 lb. to the square inch, but when cold it is advisable that the minimum pressure available should be 100 lb. to the square inch; therefore a maximum pressure of at least 300 lb. is provided for in the device, the apparatus consisting of a compressor, a reservoir, a distributor, a valve and junction box, and dashboard control mechanism. The compressor is of the two-cylinder vertical type. When fully charged the reservoir is capable of starting up the car about thirty times.

Yet another variety is the Sunbeam engine starter, which takes the form of an air engine set at the rear end of the near side above the engine crank chamber, and which, by moving a pinion into engagement with teeth cut on the flywheel, couples up the air engine with the car engine. The air-compresser itself has its crank case formed in one with the lid of the gearbox. It should be pointed out that the compressed air is not admitted to the cylinder of the air engine, which works under entirely normal conditions when rotated by the air engine, precisely as though it were being cranked by hand, though the rate of turning is much faster. Thus there is no need for valves in the cylinder heads, or of distributing valves, such as is required on the ordinary atmospheric starter; while, when the car is running, none of the parts of the air starter are in motion, both air-compresser and air engine being then disengaged from their drive.

Another variety is the Delaunay-Belleville device, which takes the form of four horizontal air cylinders opposed in pairs, and set transversely athwart the frame of the car between the two dumb-irons immediately in front of the radiator, where the appearance is not greatly improved by a block of metal, though that is doubtless a minor matter.

What is of significance is the fact, illustrated at this show, that one after another of the leading motor manufacturers are realizing that the age of hand-cranking is rapidly passing, and that serious, varied and extensive practical efforts are being made to evolve the self-starting motor car. The welcome sign is that in another twelve months every maker will be coming forward with self-starting motor cars on some principle or other, the more so in that we see already that it is possible to do this in a variety of ways, each quite practical of its kind. That is surely good enough to go on with. We can wait a few seasons before determining which shall be the ultimate type or method.

Of Mechanical Developments.

Turning now to general mechanical tendencies, changes in these are not numerous. Mors comes forward with a new type of so-called valveless motor; the Germaine firm from Belgium produces a 20-h.p. model equipped with a Daimler-built form of double sliding sleeve engine, and the 40 to 50-h.p. Peugeot car has a combination sleeve and poppet valve motor; while Argylls introduce a more popular powered single sleeve valve engine, dispensing with the junk ring hitherto deemed essential to sleeve valve engines, and Darracqs introduce two more popular rotary-valve controlled engines. It cannot, however, be said that the poppet valve engine ceases to enjoy what is practically a monopoly of patronage, while certain practices characteristic of it are being incorporated into other types. Thus, in the first place, from the very beginnings of competition work in the years when Mors and Brasier were successively supreme the cylinders were off-set in relation to the crank-shaft, a practice which we likewise find embodied in the standard design Sunbeam, which has never met a defeat in any of the many big competitions and world's records it has essayed this season. Hence it is not surprising to see that quite a number of cars—including, for example, the new Belsize, the Singer, some Peugeot models, and an Excelsior car—also have the cylinders off-set in relation to the crank-shaft, while perhaps more interesting still is the fact that in the new Daimler double sliding sleeve valve motors this system is introduced to the extent of 13 millimetres in combination with longer connecting rods and a longer travel for the sleeves, so that there is not only more perfect closing of the ports at slow engine speeds, but also a gain in smoothness of working, therefore of efficiency and absence of vibration through reduction of side-thrust and friction. One of the features of last year's practice, too, was the introduction by British makers of silent chains for working the distributing gear in the guise of cam and magneto shafts as well as water circulating pumps on poppet valve motors, a system first found possible by Daimlers with the sleeve valve engine type. This is now adopted by quite a number of poppet valve firms, including Cadillac from America, Adler from Germany, Excelsior and Métallurgique from Belgium, De Dion and Vinot from France, and Vauxhall and Swift in England, so that we see that the sleeve-valve engine is gaining from poppet-valve engine practice, and the pocket-valve engine is gaining from sleeve-valve engine practice, which is as it should be. The Darracq rotary valve, moreover, is now chain-driven.

Power Transmission, Suspension, and Coach-work.

A few years ago there was a great vogue for the worm drive, which has gained extra adherents this season, too, in the new 30-h.p. Sheffield-Simplex model, as well as the 15 to 20-h.p. Armstrong-Whitworth, which exploits the overhead type, and sundry French makers now, including the small Brasier car, the 50-h.p. eight-cylinder De Dion, and, at option, the 25-h.p. four-cylinder and the 26-h.p. eight-cylinder types, as well as the Grégoire. But it would be quite erroneous to conclude from this that the bevel drive is going out. In fact, it never looked more like holding its own, for it has now been proved possible to

make on commercial scales bevel gear that does not undergo any distortion in the process of case-hardening. It is admittedly a more efficient form of drive, provided it can be manufactured perfectly accurately in order to make it as equally silent working as the worm variety, though in the case of very small bevel gear with what one might style only a few notches on the tail of the propeller shaft, there is, as M. Brasier points out, some advantage, instead, in having the worm drive.

There is not much change in power transmission, though, particularly in the case of smaller cars, quite a number of manufacturers are reverting to the principle of superposing the gear shafts instead of placing them in a common plane. This undoubtedly makes for enhanced silence of working. In regard to braking, the areas of the surfaces have been made of still larger proportion as an almost general practice, while the means of hand adjustment is being made more ready and accessible. Daimler puts the hand brake lever in a gate, to give the driver the opportunity of choosing whether he will declutch or not when the rear wheel brakes are applied. By reason of the mania for low-seated streamline bodies, the tapered bonnet and dash, with the petrol tank placed in a dust-raising position behind the back axle, is unfortunately becoming a somewhat general practice, for it is undoubtedly a retrograde step. Happily we may regard the development as purely in the nature of an ephemeral vogue.

Suspension has not undergone many changes. Quarter elliptic, lark-heel springs are evidently going to be used fairly widely for the little cycle type of car that is coming along—as instance, Peugeot and Humber practice—among others which will be further illustrated at the Cycle Car Show to be opened on November 26th in the same building as the current one, while, though the Wolseley Company is among those that have taken out patents for cantilever springs for big-powered cars, it does not show any at Olympia, nor does Rolls-Royce, though it has standardized its cantilever sort for 1913, and has this form of suspension on its trials car outside the building. There has been, however, a considerable spread of the practice of carrying the back springs below the axle, on the system that, for worm-driven cars from below the back axle, has three big advantages, and which has long been used by Renault with bevel drive. It was standardized by Wolseleys with worm-drive some time ago, while Daimlers and Peugeot are among the firms taking newly to it, and De Dion may be described as reviving it, in that it was a standard practice of their cars in 1899, when it was abandoned because the public did not appreciate the gains of it in those early days. As the system is likely to grow, it may therefore be pointed out now that by this means, with worm-drive below the back axle, there is less movement and consequently wear of the universal joints on the propeller shaft, less vibration and easier springing on rough roads, and less wear on the back tyres, the reason being that, with the spring attached below, springs and propeller shaft are parallel and spring about the same lateral centre in the case of worm-driven cars, hence the rear axle rises and falls in a natural arc.

Lastly, there are notable developments in coach-building by Lanchester, who builds up wooden bodies on a metal frame, so that coach-work, as well as car, have frames each fitting the other perfectly; and by the Birmingham Small Arms firm, that builds up its bodies in pressings of steel, mounted and cross-tied on a metal frame and standardized in four colour schemes. The gains of both systems embrace the possibility of replacing damaged doors, panels, or coach-work parts by return of post from stock, the end of the era of the coach-builder who makes his parts fit by chopping and paring with a chisel.

Features of individual car exhibits will be dealt with in subsequent articles.

The exhibition closes on Saturday, November 16th.

THE councillors of the city of Vienna who were recently on a visit to London voted on their return home a sum of 5,000 kronen to be sent to the Lord Mayor to devote to any charitable object in the City that he pleased. Sir Thomas Crosby, in virtue of this discretionary power, has sent the whole sum to St. Bartholomew's Hospital, which at present is urgently in need of funds. The sum in English money is equal to about £206.

DISCUSSION ON HEREDITY AND DISEASE AT THE PATHOLOGICAL SOCIETY OF MANCHESTER.

AT a meeting of the Pathological Society of Manchester on October 9th, over which Mr. J. W. SMITH presided, a discussion took place on heredity and disease.

The subject was introduced by Dr. ARCHDALL REID, who, after some preliminary remarks on the nature of evidence and proof in biological questions, said:

Let us see whether we cannot reach a reasonably certain conclusion about the problem in heredity which most concerns us as medical men—namely, the problem as to whether parental disease tends to render degenerate the offspring subsequently born. By the word "degenerate" I mean innate degeneracy dependent on germinal change, of which the proof is that it tends to be transmitted to descendants.

Likenesses and differences between parent and offspring are of two kinds—inborn and acquired. The inborn likenesses and differences are due to germinal likenesses and differences. The acquired likenesses and differences arise because parent and child have developed under similar or dissimilar conditions. Inborn differences are usually called "variations"; acquired differences are termed "modifications."

Darwin noted that offspring tend to vary from their parents, and that in every species a large proportion of offspring perish. Thereupon he made the reasonable inference that there was natural selection—that is, he supposed that, as a rule, the individuals who had varied so that they were well fitted for the struggle for existence had carried on the race, while those who were worse fitted were eliminated before they had contributed their quota of offspring. On this inference he founded another equally reasonable—namely, that it was this natural selection of the fit and elimination of the unfit that had caused species to differentiate from ancestral types and from one another.

Let us see whether we cannot substitute observation for inference, and so establish Darwin's hypothesis. The evidence I am about to bring forward should particularly appeal to medical men; it is by far the most decisive evidence known to me—indeed, it is the only decisive evidence in existence. Darwin, his followers, and opponents drew their evidence from plants and lower animals. It is impossible to observe whether natural selection does or does not occur among wild types. We are not sufficiently familiar with the details of their lives. Consider our most familiar animals and plants—for instance, sparrows, house-flies, and the grasses of our lawns and fields. How little we know about them; so little that we cannot observe whether any particular kinds of individuals tend to be weeded out or to survive. But now consider our own human species with every detail of whose existence we are familiar. Man has been termed by naturalists Nature's rebel son who has escaped from selection. You know from your own experience that this is not true. You have observed many human beings perish young, even in infancy, while others survive and see their grandchildren. Of what do those who have not contributed their quota to the species die? As you know, in the vast majority of cases, of disease. What kind of disease? Common experience and public health statistics all over the world tell us mainly of microbic disease.

Now, the question arises, are diseases selective? You know from your own experience that tuberculosis is selective. You know also that tuberculosis tends to run in families, and, therefore, that the diathesis is inborn and tends to be inherited. What you observe so clearly in the case of tuberculosis you may observe with more or less clearness in the case of every lethal disease. Here, then, is natural selection not inferred but actually observed. Man is no rebel son who has escaped from selection, but a dutiful and sorely disciplined child. And not by disease only is he disciplined. Here, in this great city of Manchester, for instance, he meets a complex of lethal conditions that weed him out most stringently. Only individuals who are very resistant to the conditions which prevail in crowded places can survive in great cities, and especially in their lower quarters.

As regards Darwin's second inference, that natural

selection is the cause of evolution, it is common knowledge that every race is resistant to every disease in proportion to its past experience of that disease. If this be so, selection by diseases must have been a cause of evolution in the human race. There is plenty of evidence which demonstrates that the negroes of West Africa who have been long and severely weeded out by malaria are more resistant to it than the natives of India, who have suffered less, and who in turn are more resistant than Englishmen who have not suffered at all, or very little. Englishmen are more resistant to tuberculosis than Africans, and much more resistant than the natives of the Western Hemisphere, to whom tuberculosis was unknown before it was introduced—like measles, whooping-cough, small-pox, and many other diseases—by Columbus and his successors.

The migration of diseases to the Western Hemisphere furnishes a most instructive lesson. We in England, for instance, have been afflicted by our domestic diseases from a remote and savage past. As the population grew more settled and dense, the conditions that favour the spread of such diseases as pass from man to man grew more and more; selection constantly increased in stringency. At last no man escaped infection unless he was innately immune, or death unless he was innately resistant. The race met this increase of stringency by a corresponding evolution, and thus became capable of enduring the hard conditions imposed by civilization. Only a race which has undergone evolution against the diseases of crowds is capable of civilization. When Europeans migrated to the Western Hemisphere they introduced the hard conditions, not gradually but suddenly, to the aborigines, who could not in a few generations achieve the evolution which we had achieved only during the course of thousands of years, and at the cost of thousands of millions of lives. At once, except where defended from sudden civilization by virulent malaria, the natives of North and South America, Australasia, and Polynesia rushed swiftly to extinction. At the present day they have vanished over vast areas.

The case is very different in Asia and Africa, which have been in communication with Europe, and in which our diseases, though less prevalent, have been scourges from old time. In Asia and Africa, as a rule, we cannot colonize, we can only conquer. The natives, sufficiently resistant to our diseases, do not make way for us, and their own local diseases defend them.

The mere political results of man's evolution against disease are of almost incalulable magnitude. The human races of one half of the world are dying, and are being replaced by races from the other half. A quite disproportionate part in this great movement has been borne by our own race. It has seized on the larger part of those regions in which the aborigines were incapable of civilization by reason of their lower powers of resisting the diseases of crowds and where they were undefended by malaria.

Negroes furnish a beautiful example of evolution. Tuberculosis has prevailed among them, but, owing to climatic and other conditions, not nearly to such an extent as among us. From very early times there has been an immense migration of negroes to Southern Europe and Asia. Even in recent times the Dutch and English between them imported 12,000 negro soldiers to Ceylon, whence they never returned. Yet from the Malay Peninsula to Spain can be found no trace of negro blood. The immigrants all perished, chiefly of tuberculosis. About 400 years ago there began a large migration of negroes to the warmer parts of America, where they were taken to replace the even less resistant aborigines who had perished. Engaged in agriculture in a country where white men and tubercle bacilli were yet infrequent, they persisted, though at sore cost. As the conditions slowly grew worse they slowly evolved powers of resistance. To-day their descendants are found, even in the cities of America and Europe, though it is still said that every other adult negro perishes of consumption.

I need not multiply instances. In the time at my disposal I cannot flatter myself that I have offered very convincing proof, but I think if you will go into the evidence more thoroughly than I have had time to do here, you will find that the whole of the evidence accords with the theory of natural selection.

Many other explanations of evolution besides natural selection have been formulated. It is necessary in order to prove my case to show that these are not in accordance with the observed facts. I have time to notice only two—the Lamarckian hypothesis, which is relatively very old and is still held by many medical men, and the Mendelo-mutationist hypothesis, which is relatively new and is very fashionable with biologists.

According to the Lamarckian doctrine, the acquirements of the parent tend to be transmitted to the child, so that they reappear in the latter, not as acquirements, but as inborn traits. Of course this means that all agencies affect the race in the same way as they affect individuals. Thus it is supposed that, if you give a race plenty of fresh air, exercise, and food, and make everything splendid for its individuals, you will ultimately have a splendid race; whereas if, for instance, you crowd it into slums, you will have a degenerate race. A race exposed to malaria should on this hypothesis be rendered weaker and more liable to malaria. The Lamarckian hypothesis is directly opposed to the theory of natural selection. If the one is true the other cannot be true.

Now I have already considered crucial examples which enable us to decide between the two theories. Consider tuberculosis. What is the effect of this disease on the individual? It injures and weakens him in many ways; it benefits and strengthens him in no way. According to the Lamarckian doctrine a race that is exposed to tuberculosis should steadily deteriorate till it becomes extinct. According to the Darwinian hypothesis, the only effect which tuberculosis should have on a race is to make it more resistant to tuberculosis. Now turn to Nature, and see what has actually happened. You will not be able to find a single race in which that disease has caused deterioration, but hundreds in which it has caused an evolution of resisting power. Malaria, urban life, alcohol, and many other death-dealing agencies, also furnish crucial examples. In every instance the Lamarckian doctrine is decisively negatived.

Now let us consider the Mendelo-mutationist hypothesis. There are two ways in which a child may vary from its parents: by fluctuation and by mutation. Fluctuations are just those ordinary inborn differences which distinguish offspring from their parents and one individual from another of the same race. Mutations are sharper contrasts; they are sports—such things as hare-lip, club-foot, albinism, haemophilia, colour-blindness, idiocy, and extra digits. They are almost infinitely rarer than fluctuations. It is only true in a limited sense that the difference is one of magnitude. Fluctuations shade into one another; mutations do not. Fluctuations in parents tend to blend in the offspring; mutations behave like sexual characters and do not blend. On the Darwinian hypothesis selection is by fluctuations, evolution proceeds up a smooth inclined plane; on the mutationist theory evolution proceeds by a series of leaps. Now turn to our crucial examples. Consider tuberculosis. In this country there is every shade of difference between extreme susceptibility to the disease and extreme resisting power. We find similar shades of difference in the case of every other disease and ill condition—in fact, wherever it is possible to observe clearly, not merely to infer. Obviously, then, fluctuations are selected. Again, if the differentiation of races had occurred by selection of mutations, we should expect half-castes and their descendants to illustrate Mendelian laws in their resemblances to and differences from their parents. There is no evidence of this. Racial characters always blend.

The conclusion we reach, finally, is that Nature selects fluctuations, and that this selection is the cause of evolution. And now, from the medical point of view, we reach the heart of the matter. What is the cause of variations? Do they occur because the experiences undergone by the parent alter the germ plasm, or do they arise spontaneously in the sense that they are the results of the normal life, growth, and division of the germ plasm? For example, does parental disease cause offspring to be different to what they would otherwise have been, or do such innate changes as we see in the offspring arise quite independently of anything which happened to the parent? Consider such a disease as malaria, which in West Africa attacks the whole population, and for prolonged periods soaks every germ cell in virulent toxins. If malaria is a cause of variation it must everywhere produce the same kind of

variation. Now, obviously, if all the children in a generation vary from their parents in the same direction, natural selection is abolished. There remains no material for choice. The race of negroes must then drift in a given direction till it becomes extinct. The opposite is what happens. As we have seen in the case of malaria and every other infective disease, the race does not degenerate but undergoes protective evolution. We must conclude that variations are normally spontaneous.

Most biologists believe not only that natural selection is the cause of evolution, but also that variations are commonly caused by the direct action of the environment on the germ plasm. But these suppositions are contradictory; they cannot both be true. A race that drifts, that constantly degenerates, cannot undergo evolution. Doubtless the germ plasm is constantly injured by such things as malaria, but you must think of it as a living entity which, like other living things, tends, if it survives, to recover from injury. Life could not persist on earth, could not change in adaptation to changing surroundings, unless variations were commonly spontaneous. It is quite another thing to say that they are always spontaneous. It is quite probable that malaria, for instance, does sometimes alter the germ plasm of individuals so that it does not recover and does not die, and so causes variations. Such permanent alterations are injuries from which no recovery is made. Doubtless different strains of germ plasm vary in resisting power, and the weaker are unfavourably altered and so weeded out. There is thus a Natural Selection which tends perpetually to render the germ plasm insusceptible to the influences which act on it from without.

Professor HICKSON said he had accepted the invitation to take part in the discussion with hesitation on account of his ignorance of disease, but he would like to express some views from a biological standpoint. In the first place, it had struck him in reading papers on inheritance in man that the evidence was often unsatisfactory in one way or another. The reason seemed to be that man presents many great difficulties for such investigations. In one striking characteristic he differed from all other animals—in the extraordinary length of his period of juvenility. Civilized man reached physical maturity at about 15 years of age, mental maturity much later. Man appeared to require for his proper development a long period in which he might be influenced by his environment. In his adult form man was much more a creature of his environment than other animals. It thus became extremely difficult in man to distinguish between innate and acquired characters. Another difficulty was that man was a very slow breeding animal. It was thus difficult to get a sufficiently large number of cases of transmission of a particular character. Few statistics included more than three or four generations, or 100 to 150 individuals. In studying heredity in peas or in mice biologists were not satisfied with less than several thousand individuals. There was a third difficulty, not peculiar to man but common to all placental mammals, in determining whether a character was really innate or not. There was the possibility of the transmission of characters by the placental blood. Such characters were, of course, not innate. This term could only be applied to characters transmitted through the germ cells. For these reasons he was always sceptical of statistics from human beings bearing on the transmission of acquired characters. On another point he must differ from Dr. Reid. He thought it very doubtful if a hard and fast line could be drawn between fluctuations and mutations. There were, undoubtedly, mutations which were transmitted without blending, but there were others in which there appeared to be only a difficulty of blending. In the case of an extra digit blending could only mean the production of an extra half-digit. Now, in experiments on poultry an ardent Mendelian had shown that the transmission of an extra toe obeyed Mendelian laws, but on analysis of the figures it appeared that many of the extra toes were only half-toes which could equally well be interpreted as blending. So far as he could judge, he was inclined to agree with Dr. Reid that there was no evidence of the transmission of acquired characters of disease in the human race.

Professor WEISS said that his studies were even more remote from human pathology than those of Professor Hickson; at the same time some interesting experiments had been made on immunity to disease in plants which did not quite agree with Dr. Reid's conclusions regarding immunity in man. Professor Biffen of Cambridge had experimented with certain disease-resisting varieties of wheat, and it appeared from these investigations that immunity to the rust disease of wheat did not present itself in a series of gradual fluctuations, but that immunity was probably due to the absence of a certain factor—possibly some chemical substance—which, when present, rendered the wheat susceptible to disease. The character which conferred immunity in this plant was therefore more like a mutation than a fluctuating variation. Experiments of this kind were, however, very difficult, and it seemed likely that the problem was not so simple; the more recent investigations of Nilsson Ehle in Sweden appeared to indicate that resistance to disease was due to a combination of several factors. At all events, Nilsson Ehle had obtained, by crossing, some races of wheat which were more resistant than either of the parental forms, and this seemed incompatible with the view that immunity depended upon a single inheritable factor.

Dr. REYNOLDS said that it was in the affections of the neuro-muscular system that the best examples of the hereditary transmission of disease were found. The insanities, the neuroses, and the muscular heredity diseases should be studied in this connexion, and attention should not be confined to the bacterial and infective diseases. Dr. Reid doubted whether acquired characters could be handed down by heredity. To deny this seemed a most dangerous and immoral doctrine, as it would mean a total disregard of environment. There was not the faintest doubt that syphilis could be handed down. One result of denying the possibility of acquired disease being handed down would be that there must have been an extraordinarily bad race to start with. Where had diseases come from? Mental diseases tended gradually to eliminate families. Where was the fresh stock of insane people coming from? It was surely possible to create insane families. "Take," he said, "the children of parents as stolid and as little neurasthenic as you like. One of these takes to drink, or some other vice, or to living in a close atmosphere, or is otherwise exposed to unfavourable conditions, and the offspring of that particular backslider may begin to have some neurosis which may be passed on to his descendants." There must, Dr. Reynolds thought, be an acquired heredity of disease. Dr. Reid asked if it were possible to conceive that a degenerating race could evolve by natural selection. Probably not, but in one sense degeneration and evolution went hand in hand; for was not the degeneracy of the feeble stock necessary for its extinction and for the selection of the better stock?

Dr. MUMFORD said that he had been engaged for a number of years in trying to form an estimate of the comparative effect of heredity and circumstance in causing a manifestation of morbid qualities. He had studied a number of families through five or six generations. It was necessary that such families should be related, and should include maternal as well as paternal ancestry over more than two generations. He had succeeded in getting sufficient information concerning about a hundred such related families, and was able to note the onset of epilepsy, alcoholism, consumption, and heart failure, and also to watch the conditions under which these disappeared. It seemed to him that outward circumstances and conditions had a very powerful effect on healthy as well as on morbid growth and that heredity influences were often capable of wide modification. Thus, if a part of a family was much reduced in circumstances by the early death of the father, or other conditions arose which impaired family nutrition, a diminution in family vigour occurred and morbid conditions appeared. His studies led him to question the correctness of Dr. Reid's view that a nation becomes sober largely because it becomes immune to alcoholism by the alcoholic families dying out. It seemed to him more probable that generations became more sober because wiser methods of bringing up children were adopted, and fuller social interests prevailed so that the nervous system was less under the sway of uncontrolled emotions. Early death from heart failure at the age of 45 to 60 seemed very largely hereditary, but even here the nutritional power on the part of the mother had great influence in modifying

the tendency, for the children who composed the middle of a family and were born during a period of enhanced nutritional power in the mother were often stronger and exhibited to a less extent any hereditary weakness than the firstborn or those born when the maternal nutritional power was decaying. Maternal nutritional power, capable of raising the level of vigour of a generation sufficiently to throw off morbid heredity, was often accompanied by longevity.

Dr. Reid, in reply, said he could not agree with Professor Hickson that it was more difficult to study heredity in man than in the lower organisms. The observer's power of study was commensurate with his familiarity with the object studied. As to statistics, he had appealed to the present and past of nations and races, and not to two or three generations. He held that mutations were transmitted like sexual characters—namely, without blending—and that very many examples of Mendelian inheritance were sexual traits.

THE PARLIAMENTARY COMMITTEE ON PROPRIETARY MEDICINES.

On October 30th Dr. W. E. Dixon, Professor of Materia Medica at King's College, Cambridge, gave evidence of a general character on behalf of the British Medical Association. He spoke of the harm done through the administration of proprietary remedies without previous expert diagnosis. Referring particularly to headache powders, Professor Dixon said that such a dangerous drug as acetanilide ought not to be administered in any circumstances. The witness endorsed the opinions expressed by previous witnesses that a vendor of a proprietary remedy should be held responsible for the claims made on behalf of the preparation, and that the rights of a firm in a particular compound invented by them should only remain in force for a limited period. He held that the present law was greatly in need of amendment, but at the same time he felt that if the law as it existed were enforced the evils might be very considerably mitigated. The Merchandise Marks Act gave power to public authorities to prosecute in cases in which representations were made which were obviously fraudulent. The powers conferred by this Act should be made compulsory instead of optional. With regard to the declaration of the ingredients on the label, he feared that in the case of the more objectionable preparations the proposed law would be circumvented by the use of special names signifying nothing. He regarded the proposal as advantageous because he thought it was the mere secrecy of proprietary medicines that impressed the public.

Replying to Dr. Chapple the witness said he would regard it as a fraudulent claim to state that any remedy was a certain cure for a disease.

Replying to further questions, the witness said that no useful drug had been introduced into medicine in the last thirty years except through the laboratories and pharmacologists. It was impossible for a layman or even for an ordinary medical practitioner to make these discoveries. Results could only be arrived at under absolute conditions and under control.

In reply to Mr. Lawson, the witness said he did not condemn all proprietary medicines without respect to the claims they made. There were genuine and legitimate preparations on the market in respect of which the proprietors were entitled to the benefit of the skill exercised in their production. Various cod-liver oil emulsions came into this class. He would put the vendor of secret remedies on the same level as the qualified chemist and make him responsible for the purity of the ingredients used.

On the question of the number of similar synthetic preparations known under different names, the Chairman asked why doctors did not prescribe the formulae given in the *British Pharmacopoeia*. Professor Dixon replied that, in his opinion, medical men ought to do this, and the younger generation would do so.

Mr. O'Grady: There are about fourteen million people insured under the Insurance Act. Are not these the people who are the principal purchasers of proprietary remedies?

Professor Dixon: I do not agree with that. The clergy are as bad as any. (Laughter.)

Mr. Lawson: And the British Medical Journal recently discussed whether Nonconformists were not specially addicted to secret remedies.

Mr. O'Grady: If, as a result of the Act, all these people go to a doctor, will not that do away with the necessity for special legislation in respect of quackery?

Professor Dixon: You are assuming that a man goes to a doctor first and gets well. But suppose he goes and does not get well—that the doctor tells him he is incurable. No one ever gives up hope, and when a man sees an advertisement, "I will cure cancer," he reads the testimonials and wastes his money in trying the preparation.

Mr. Glyn Jones: Is it right that we should be indebted to private enterprise for our knowledge of new drugs?

Professor Dixon: We are not entirely, I am glad to say. The pharmacological departments of our universities have contributed their quota. In reply to a further question, the witness said he did not agree that the rank and file of the medical profession did not want the patient to know what was in the prescription given him.

The Chairman: Is the fact that doctors' prescriptions are still written in Latin a survival of the day when doctors sought for their prescriptions the same secrecy that proprietary medicine vendors seek to-day?

Professor Dixon: You are asking me a historical question. My answer would be that I am not an authority on that.

The Chairman announced that the Committee had received from the United States Government a copy of the new amended American Food and Drugs Act which became law on August 20th last. In this Act misbranding was made an offence—a clause providing that if the article contained any statement on the package or label regarding the curative or therapeutic effect of the preparation which was false or fraudulent, the vendors were liable to prosecution.

The witness said that this appeared to him a distinct step in the right direction.

After the Committee had consulted in private for some time, the Chairman stated that no further medical evidence would be called for the present, but that at its next sitting the Committee would hear a witness on behalf of the Proprietary Articles section of the London Chamber of Commerce.

MEDICAL SCHOOL FOR TUBERCULOSIS.

The new medical school of the Royal Hospital for Diseases of the Chest, City Road, was opened on October 17th, when Sir William Osler took the chair, and Professor Nietner of Berlin delivered an address on the modern combat against tuberculosis amongst children.

The Council and staff of the Royal Hospital for Diseases of the Chest consider it to be their duty as a special hospital for diseases of the chest to provide training for medical officers of tuberculosis dispensaries and sanatoriums, and more specially for general practitioners, and through the generosity of one who fully realizes the great value at this juncture of affording facilities for such training they have been able to build an out-patient department, equipped with all details for research work and scientific instruction on modern lines, and with special accommodation for a tuberculosis dispensary. But it is not only hoped to constitute the hospital as a training centre for those who wish to qualify themselves for dispensary and sanatorium appointments, but also as a special training centre for tuberculosis nurses. In Germany it has long been recognized that efficient training is indispensable for the domiciliary work connected with the dispensary system; above all, training in the collection and interpretation of data. The properly trained observer can be relied upon to procure the right sort of information, and this will conduce to economy, both of financial expenditure and of effort. Arrangements are being made to receive nurses for two years' training or for shorter courses of post-graduate teaching. In these circumstances it was held that no one could be better fitted to give the inaugural lecture than Professor

Nietner, who is the General Secretary of the German Central Committee for the Prevention of Tuberculosis. Founded in 1895 by the Reichskanzler of that day, Fürst von Hohenlohe, it was designed to link up all schemes and associations throughout the German Empire which have as their object the prevention of tuberculosis. The President is the State Secretary of the Interior for the time being. The council includes representatives from the Federal States, the Imperial Board of Health, and the Imperial Insurance Board, as well as a number of high officials and medical experts. The Central Committee co-operates closely with the various invalidity insurance institutions, by which such local organizations as bid fair to prevent permanent ill health are liberally supported. The State gives a yearly grant of £3,000. The working of the Central Committee presents an admirable object-lesson in organization and the avoidance of overlapping. It is the arduous duty of the General Secretary, in his advisory capacity, to travel throughout the empire, and all new local schemes and associations are carefully studied by him.

Professor Nietner's Address.

Professor NIETNER stated that when the Central Committee was first established its efforts were mainly directed to the care for the still curable diseases, the means for so doing being amply provided by the invalidity insurance institutions. But latterly the trend had become ever stronger towards prophylaxis. It was realized that prevention must begin with the child. This truth was practically ignored during the early history of the campaign against tuberculosis in Germany, for little was definitely known about the incidence of this disease in childhood, and the latent tuberculosis of infancy was almost an unknown factor. Kirchner in Germany first pointed out that though the incidence of tuberculosis amongst adults was steadily decreasing in Germany, the children of the nation did not participate in this decrease.

Professor Nietner said that the researches of the last ten years have brought to light facts that point to the conclusion that in a very large majority of cases infection occurs during childhood, and, indeed, in the first years of life. Hamburger declares that 90 per cent. of all children up to the completed 12th year are infected. Schlossman has gone so far as to say that tuberculosis is a true children's disease, is acquired during childhood, and must be prevented, treated, and healed during childhood. The fact stands beyond a donbt that in by far the greater number of cases the source of infection can be traced to the human subject suffering from "open" tuberculosis, and that infection is acquired through the close intercourse resulting from family life within the walls of the home. Only those preventive measures can, therefore, hope for success which keep this fact constantly in mind. But to prevent the child from becoming infected in his home without weakening those family ties and responsibilities which are so essential to the moral health and true happiness of the nation is a social problem of the utmost complexity. Professor Nietner deprecated drastic schemes for completely separating the child from his family, though every care should be taken to safeguard it in its own home. Professor Nietner denied that tuberculosis was a "school disease," and maintained that the school could not justly be held responsible for the spread of infection. He attached the utmost importance to the careful organization of the school medical service in the battle against tuberculosis, and said that to the school doctor alone was the power given to prevent latent tuberculosis developing into active disease in the children examined by him, and kept under his watchful supervision. With a complete State organization of school medical supervision, the discovery and recovery should be possible of a large number of children who might otherwise become later on a serious strain on the economic resources of the nation by developing active tuberculosis during the wage-earning period of life. Germany could boast of a very large number of agencies for promoting the health and welfare of children, and these were closely linked with the school medical service. In the sanatoriums for children the practice of imparting some school teaching was rapidly gaining ground. Finally, Professor Nietner said that he attached great value to the use of tuberculin in the treatment of tuberculous children, and claimed good results from it if properly administered.

CHARING CROSS HOSPITAL MEDICAL SCHOOL AND THE UNIVERSITY OF LONDON.

UNIVERSITY LABORATORIES OF PUBLIC HEALTH.

THE final step in the negotiations which have been conducted for some time between the authorities of King's College and of Charing Cross Medical School was completed by the handing over of the laboratories of public health and bacteriology to the university on Thursday, October 31st. The proceedings were rendered the more interesting by the fact that the Huxley Lecturer, Professor Simon Flexner, the Director of the Rockefeller Institute in New York, was asked to declare the laboratories open, and to hand them over to the university.

The DEAN OF CHARING CROSS MEDICAL SCHOOL (Dr. William Hunter) gave an account of the changes. He pointed out the great advantage to the medical education of students which resulted when the preliminary and intermediate subjects in science were studied in the university. Last year the Charing Cross School, after due consideration and on his advice, had transferred the teaching of these subjects to King's College, and the rooms hitherto used for instruction in physics, chemistry, biology, anatomy, and physiology had thus become available for other purposes. The school would not lose by this transference, and the university undoubtedly reaped an advantage. In order to bind the medical school and the university more closely together, and to further the interests of university education, he conceived the idea of devoting space thus set free to public health and bacteriological laboratories, which, as part of King's College, would relieve the congestion in the college itself, and bring the Charing Cross School into intimate association with the university. The professorial and teaching staff of King's College in these subjects had been transferred to the new laboratories, where senior students and qualified public health workers would be received.

Professor FLEXNER dwelt on the importance of public health teaching and investigation. Bricks could not be made without straw, and since the Governments of both America and England did not supply straw enough to make the scientific bricks with, it devolved on private initiative to supply the want. One advantage of the amassing of colossal fortunes in the United States was that it had been rendered possible for the rich to carry out this function. Carnegie had given 25 million dollars for the foundation of the Carnegie Research Institute in Washington, and Rockefeller had given 10 millions for the institute in New York. If he did not misinterpret the signs of the times, further large financial assistance would be given to the research in preventive medicine in his country. He hoped that England would take this lesson from the new country. He then formally handed over the laboratories to the university.

The Hon. W. F. D. SMITH, Chairman of King's College, who received the gift in the name of his college, spoke of the benefits of reciprocity between the two institutions. He looked forward hopefully to the carrying out of a large amount of important research and post-graduate work in the laboratories.

Principal HEADLAM, of King's College, spoke of the extremely pleasant nature of the negotiations which had led to the result now being celebrated. The step would, he believed, lead gradually to the building up of a much greater institution.

The PRINCIPAL OF THE UNIVERSITY OF LONDON, who also spoke, addressing himself to the upper rows of students, said that the future of the medical school and the university lay in the hands of the gods—the students themselves.

THE LABORATORIES.

The building in Chandos Street is but thirty years old, and it was found that considerable structural alterations would not be required for the adaptation of the laboratories to their new purpose. Some new windows have been made, and the internal equipment adapted to its new purpose. On the north side of the buildings the upper floor is now devoted to bacteriology. There is a large well lighted and adequately equipped laboratory to accommodate about thirty students for the teaching of advanced

bacteriology. Professor R. Tanner Hewlett, who is in charge, has every reason to be satisfied with his new teaching quarters, and the class will find more room and better surroundings than they had in the old laboratories in King's College. Adjoining, there are the research laboratory, a fair-sized room with the makings of a good home for original investigation, and the accessory rooms, including a large one for the Profesor of Bacteriology. Although not included in the university set of laboratories, the lecture theatre, and the *post-mortem* room, with its dependent apartments for operative surgery work, bacteriological, and other investigations, are conveniently at hand.

On the floor below, also to the north, is the Public Health Department of King's College under Professor Simpson. The laboratory for chemical analysis, the adjacent rooms, and the theatre, which leads off the main laboratory, are all admirably suited to the routine teaching of sanitary science. The accommodation for students here is approximately the same as in the bacteriological department, and the apparatus and other equipment are entirely satisfactory. In the basement a room for photomicrography has been arranged. We congratulate Dr. Hunter on the completion of a work of high importance for medical education.

COMPLIMENTARY DINNER TO PROFESSOR FLEXNER.

The occasion of the delivery at Charing Cross Hospital by Professor Simon Flexner of the Huxley Lecture, published in this issue, was seized by the students, present and past, of the hospital to entertain him, as chief guest, at their annual dinner on October 31st at Gatti's Restaurant. Over 160 members of the Charing Cross Medical School and their friends gathered together to show their appreciation of the great American bacteriologist, who, as he afterwards said, received so many expressions of appreciation that he was almost overcome. Dr. MOTT presided.

Dr. MOTT, in proposing the toast of "The Hospital Medical School," recalled the greatness of the school, as reflected in many eminent men. Huxley, Wharton Jones, Livingstone, Joseph Fayrer, and many others were recalled from the past, while a gold medallist at the London M.D., and others who had achieved greatness in the present generation also came in for their share of praise. In referring to Dr. Hunter's work and the relations of the school with King's College, he said that the most important factor of a laboratory was its cerebral equipment. He was satisfied that Charing Cross was safe in this respect.

Mr. DUFF, the Chairman of the Council, and Dr. HUNTER responded, and the latter sketched the changes which had come over the hospital since its foundation close on 100 years ago.

Dr. GALLOWAY, in proposing "The Guests," related how in days long past a Hermitage of St. Catherine had stood almost where the hospital stands to-day. In this hermitage hospitality was given to wandering Welshmen. On the other side of what was now the Strand a Priory of St. Mary was founded by one William Marshall, who was an Irish-Welshman. This hospice was noted for its hospitality to foreigners, chiefly from Gascony, Spain, and the Pyrenees. Though Henry VIII disturbed these admirable arrangements, the practice of hospitality, at times voluntarily accepted and at times not so, was still continued at Charing Cross. Among the guests whom he selected for special honour he named Dr. Sandwith, Colonel Skinner, Professor Muir, Mr. Duff, Mr. Verity, Dr. Oxford, Dr. Headlam, and Professor Flexner.

Professor FLEXNER, who replied, spoke of his connexion with the Johns Hopkins University and Hospital, and with the Rockefeller Institute, and of his indirect connexion with Charing Cross Hospital. In thanking them for all the hospitality and pleasant things which had been said of him, he added: "And for your forbearance, for I have given you a stiff dose to-day, and you took it well."

In his reply to the toast of "The Chairman," Dr. MOTT said that at last the preliminary arrangements had been completed for the establishment of a hospital for acute mental disease, which had been rendered possible by the generosity of Dr. Maudsley. He hoped that some day a diploma in psychiatry would be established.

LITERARY NOTES.

AMONG other books announced by Messrs. Smith, Elder and Co., for publication on November 14th, is *The Land that is Desolate*, which is said to contain Sir Frederick Treves's impressions of travel through the Holy Land as it is to-day.

Dr. F. M. Sandwith, the Gresham Professor of Physic, has prepared for the Research Defence Society a pamphlet on Sleeping Sickness. It will be published by Messrs. Macmillan and Co.

To the *Edinburgh Review* for October Mr. Victor Plarr, the Librarian of the Royal College of Surgeons of England, contributes an interesting article containing some unpublished letters of Sir Walter Scott to Joanna Baillie. The "Baillie Letters," now in the Library of the College, have already, says Mr. Plarr, been in great part reprinted. Letters of Pope, Swift, and Arbuthnot, which form part of the collection, were published by Mr. Aitken in 1892, in his *Life and Works of Dr. John Arbuthnot*, while many of Scott's letters to Joanna Baillie—who, it may not be superfluous to remind the present generation, was a poetess held in high esteem in her day—were incorporated in Lockhart's *Life*, or in Mr. Douglas's *Familiar Letters of Sir Walter Scott* (1890–92). There remain, however, some twenty of Sir Walter's letters to Miss Baillie and other members of her family, which seem to have escaped the attention of editors, probably, Mr. Plarr conjectures, because Scott's handwriting was difficult to decipher. Joanna Baillie, whose house at Hampstead was a meeting place during more than half the nineteenth century of literary artists and literary men, was the daughter of a former minister of Bothwell on the Clyde, whose wife was the sister of John and William Hunter. Matthew Baillie was her brother. We are promised further references to him in a subsequent article. The present one deals almost entirely with Scott's efforts in connexion with the production of plays by Miss Baillie, who, as a dramatist, was regarded by him and many of her contemporaries as second only to Shakespeare!

The College of Physicians of Philadelphia announces that the next award of the Alvarenga Prize, amounting to about £36, will be made on July 14th, 1913. Essays intended for competition may deal with any subject in medicine, but they must be unpublished. They must be typewritten in English or accompanied by an English translation, and received by the secretary of the college on or before May 1st, 1913. Each essay must be sent without signature, but must be marked and accompanied by an envelope bearing the same mark and containing the name and address of the author.

The *Pall Mall Gazette* of October 24th supplied a notable example of things one would rather have expressed otherwise. Speaking of the Lister Memorial our contemporary referred to the creator of modern surgery as the "great septic reformer"! Social reformers are often septic enough—in appearance at any rate—but it would be enough to make Lister turn in his grave if he could hear himself described as "septic."

Professor Abel, who has hitherto edited the *Journal of Pharmacology and Experimental Therapeutics*, has asked Professor A. R. Cushny to undertake the duties of joint editor, and has invited a number of representatives of British pharmacology to serve as associate editors. Among them are Sir Lauder Brunton, Professor Cash, Dr. W. E. Dixon (Cambridge), Professor S. Flexner (Rockefeller Institute for Medical Research), Sir Thomas R. Fraser, Professor J. N. Langley (Cambridge), Professor C. R. Marshall (St. Andrews), and Professor R. Stockman. By this arrangement, the pharmacologists of Great Britain will have an equal representation in the *Journal* and an equal interest in its future progress. The Williams and Wilkins Company, of Baltimore, the present publishers of the *Journal*, have arranged with the Cambridge University Press to issue the *Journal* in this country. The *Journal* is issued bi-monthly. Each volume consists of six numbers and contains over 500 pages. Three volumes have already been published, and the arrangement came into effect in vol. iv, which commenced in September, 1912.

British Medical Journal.

SATURDAY, NOVEMBER 9TH, 1912.

THE CHANCELLOR DEFINES.

It may be gathered from the motion proposed by Dr. Beaton at the meeting of the Council on October 31st, as reported in the Supplement this week (p. 500), and from some of the letters recently received and also printed in the Supplement, that some members of the Association are considering a third course, which they regard as intermediate between the alternatives contained in the antepenultimate paragraph of the report of Council to the Divisions and Representative Meeting. The motion was in effect to invite the Representative Meeting to take into consideration the recent offer made by the Chancellor of the Exchequer, on the ground that it affords the British Medical Association an opportunity of conferring with him and the Commissioners as to the points on which the demands of the profession have not yet been met.

The first alternative presented in the Council's report is to accept the scheme embodied in the Act, the Provisional Regulations, and the Statement of the Chancellor of the Exchequer to the Advisory Committee on October 23rd, subject to the consequential amendment of the Regulations, and subject also to the conditions and safeguards set out in paragraph 114 of the report of the Council. If the Representative Meeting should be disposed to consider this alternative course, it may well decide that the necessary conditions are not adequately set out in paragraph 114. This is the opinion of some of our correspondents, and their view will probably be shared by many silent members. The third course proposed to the Council was to indicate to the Divisions and the Representative Meeting that there were three possible courses—that is to say, as it was tersely put, "to accept, to confer, or to reject." The Council did not accept the proposal to put this third course into its report; the argument that it was undesirable for it to appear to give a lead to the Divisions and the Representative Meeting having perhaps most weight.

Some consequences which would appear necessarily to follow acceptance of the middle course demand careful consideration. It would mean negotiation. The negotiation would have to be carried on, not with the Commissioners, but with the Government—that is to say, practically, with the Chancellor of the Exchequer, who might, if he were convinced that it was necessary, cause the Insurance Commissioners to take action under Section 78 of the Act.

If negotiations are to be reopened, it must be with the Government. Mr. Lloyd George, the responsible member of the Government in this matter, has said that if he is to negotiate, the ambassadors of the profession must have full powers. Only the Representative Meeting can appoint such plenipotentiaries. It could, of course, give them strict and definite instructions, and would no doubt do so; but it seems that if it is to proceed on this line it would be bound to allow something to their discretion—some margin for compromise and for adjustment of details.

During the course of the discussion by the Council of that part (B) of the report which contained a comparison of the Regulations as to medical benefit with the cardinal principles and the decisions of the State Sickness Insurance Committee attention was drawn to a considerable number of points in the Regulations and in the statement of the Chancellor of the Exchequer on October 23rd which were ill-defined or ambiguous. The Chairman of Council pointed out that the State Sickness Insurance Committee had considered these matters and felt that there were several points which needed elucidation, but that they could only be cleared up by the Chancellor or the Commissioners. The Committee had considered itself precluded by the resolution of the Representative Meeting at Liverpool from getting that information by way of application to the Chancellor or the Commissioners. After carefully considering the terms of the resolution, the Council, believing that it was its duty to be in a position to give the Special Representative Meeting the fullest possible information, authorized the Chairman of Council and the Chairman of Representative Meetings to address a letter to the Chancellor of the Exchequer asking for further information as to certain points which were regarded as obscure. The letter addressed to the Chancellor of the Exchequer and his reply are printed in the Supplement (p. 500 et seq.), together with a brief letter received as we go to press from the Chairman of the Joint Committee of Insurance Commissioners with reference to certificates, and to medical attendance given by a deputy in the unavoidable absence of the practitioner.

It will be seen that the points as to which the Council asked for specific information were the nature of the increased services to be required from medical men who may consent to serve on the panel; a clear definition of medical benefit, with a distinct indication of the services which will not be required from practitioners on the panel; the method proposed for the provision of mileage, especially in rural districts; the exact scope of the "abnormal drug fund;" the scope of the restriction with regard to the employment of assistants, and the proposed capitation grant for non-institutional treatment of tuberculous insured persons. In dealing with the amount of remuneration, the letter called particular attention to Regulation 28 ("amount applicable for payment of practitioners"); to its provisions strong exception has been taken by the profession. The Council emphatically adopted the view that payment for mileage should come from some centralized fund, pointing out that if a deduction were made in respect of mileage from the amount available in each insurance area considerable injustice might result. In a mixed urban and rural district rural practitioners would absorb much of the fund in mileage, while in a very scattered district mileage might absorb nearly the whole of the fund.

In his reply, the Chancellor of the Exchequer defines "increased service" as an increased service compared with that obtained under many contract practice systems in the past, and with that which the majority of the insured, in view of their restricted means, could afford to obtain on the lines of private practice. It is further explained to be such service as the best opinion in the profession itself would expect from a general practitioner in his ordinary work. The intention to appoint inspectors is maintained, but an undertaking is given that such inspectors shall be medical.

With regard to modern methods of exact diagnosis, it is stated that practitioners on the panel will not be expected to undertake work beyond the ordinary scope of general practice. If a practitioner finds more elaborate methods of diagnosis necessary, his duty will end in advising the patient as to the steps he should take, and the Government hopes that in so far as facilities in the shape of expert

assistance do not now exist it will be able to encourage their development and organization. As to records, the Chancellor of the Exchequer indicates an intention of supplying free of cost to all practitioners on a panel books containing special forms, similar to visiting lists in ordinary use, showing the name and address of the patient, the name of the society, the number of visits, age, sex, and nature of illness. The confidential nature of such records is recognized, and it is proposed that they shall be open to inspection only to those officers within whose specific duties it would fall to deal with them. With regard to the definition of medical benefit the Chancellor's contention is that the range of services expected should be defined only in general terms. He insists on a distinction between two different kinds of extra services—those which fall within the scope of medical benefit and those which do not. Specialist's services, not rendered by practitioners on the panel, are stated to lie outside the scope of the arrangements to be made by the Insurance Committees. As to services of a special character rendered by a general practitioner, the Chancellor adheres to his scheme, which would include payment for all such special services in the capitation sum set aside for the remuneration of the medical profession, though that capitation sum could be distributed on various different systems, as indicated in the second part of the first schedule of the Regulations.

With regard to the amount of remuneration, it is stated that Regulation 28 is now being redrafted, and that, taken with the conditions of the new grant, it will secure that a sum equivalent to 7s. per head of insured persons (including the sanatorium 6d. for domiciliary treatment of tuberculous persons) shall be available for the remuneration of medical practitioners on a panel throughout Great Britain. It will be applied solely to medical remuneration "without deduction for drugs or administrative expenses or any other purpose," but mileage is to be paid out of it. The possibility of establishing a central fund for mileage is admitted, but it would appear that the present intention of the Government is that this fund should be formed by way of deduction from the total amount available for medical benefit; this, we take it, is not the sort of centralized fund the Council desired to see established.

It is stated that the "abnormal drug fund" is to be used to prevent the extra 6d. for drugs being drawn upon when the extra demand for drugs was clearly due to abnormal sickness. The fund would be available not only in cases where that 6d. had been exhausted in the provision of drugs but also in cases in which it had not been so exhausted. With regard to assistants, the Chancellor holds that the Regulations in conjunction with the free choice of doctor by patient will afford proper latitude for the employment of assistants while tending to check any abuse. The domiciliary treatment of tuberculosis of insured persons is defined as such amount and kind of attendance as in the best interest of the patients can be given by the general practitioner himself. The 6d. from the sanatorium fund is to be used solely for the remuneration of the doctor, without deduction for the provision of drugs or for any other purpose.

Mr. Masterman's letter does not appear to add much, if anything, to the information already in the possession of the profession.

A perusal of the full text of the Chancellor's letter will, we think, show that while it clears up some important points it leaves others still in need of further and better definition.

ILLNESS AND INTELLECT.

It has often been noted that much of the world's work is done by invalids. It is not to men of healthy digestion and restful temperament, "such as sleep o' nights," that we owe the great revolutions that make human history. In the sphere of action we find that Caesar, Mahomet, and Napoleon were epileptic; Augustus was a valetudinarian; Luther's last years were disturbed by almost constant ill health; Calvin was in himself almost a pathological museum. Richelieu was ailing during the greater part of his life, and his last years were embittered by some painful and loathsome disease of the rectum, which did not, however, till near the end interfere with his mental activity. Chatham suffered much, even in boyhood, from gout—the disease of statesmen, as it has been called. Our own Sydenham was a prey to the same disease, and sought comfort in the thought that many wise men were subject to the same infirmity. Macaulay's hero, William of Orange, was a martyr to asthma; Frederick the Great was of delicate constitution and frequently ill. John Hunter, who had his full share of the irritability of genius, died of angina.

Passing from the field of action to science and literature, we see how the fixity with which Newton "intended his mind" on his problems made him nervously excitable and almost shook his reason; how Darwin was prostrated by two hours of work daily, and obliged to lie on his back the greater part of the day, and seek relief from intellectual tension by listening to the reading of novels. We see Swift's life overshadowed by the fear of insanity which finally claimed him as its prey. Johnson was scrofulous and melancholic; he said of himself that he had been mad all his life—at least, not sane. Scott dictated the *Bride of Lammermoor* during such intervals of lucidity as were left him when taking large doses of opium for agonizing "spasms." Havelock Ellis, in his *Study of British Genius*, says that by analysis of the records in that literary *campo santo*, the *Dictionary of National Biography*, he found that 15 per cent. of men of letters and 16 per cent. of poets suffered from ill health during the active part of their lives, and it is not rash to conjecture that this does not represent anything like the whole truth.

The connexion between genius and nervous disease has been recognized from the time of Aristotle, and although few at the present day hold the theory in the crude form enunciated by Lombroso and his disciples—that genius is a form of neurosis—it is generally admitted that men of genius frequently come from families with a neurotic taint, even if no such weakness be discernible in themselves. Among those who were actually insane for longer or shorter periods are Pascal, Rousseau, Cowper, and Tasso, to mention only a few. Shakespeare is by some numbered among the "neuropaths"; Gustave Flaubert was epileptic; Chateaubriand has lately been described by his doctor as a marked example of hysteria. Of the eccentrics the name is legion. It can easily be understood how genius is connected with instability of the nervous system, and how this instability may be increased by the strain of production or overwork.

The association of gout with intellec is so frequent that it can scarcely be accidental. The disease manifests itself, roughly speaking, in one of two forms according as the poison concentrates its attack on the joints or is diffused throughout the system by the blood. In the latter case the brain is affected,

like other parts, and this gives rise to a state of general gloom. On the other hand, when the joints have to bear the brunt of the assault, the mind seems to undergo a kind of lustration, the effects of which show themselves in increased clearness and vigour. It may be said that arthritic gout has something of the action which Falstaff attributes to sack: It dries all the "foolish, dull, and crudy vapours" which environ the brain and "makes it apprehensive, quick, forgetive [inventive], full of nimble, fiery, and delectable shapes; which delivered over to the voice (the tongue) which is the birth, becomes excellent wit." One might indeed argue that Falstaff's own wit was due to gout; does he not say, "A pox of this gout"? On the other hand, it is at least conceivable that genius may be the cause of gout. The disease is not unlikely to be induced in persons of strong frame by the want of exercise due to conditions of literary toil, perhaps aided by excesses in eating and drinking. It is nothing to the purpose to say, as Mr. Havelock Ellis does, that many of the most gouty persons in his list of "intellectuals" have been extremely temperate both in food and in drink. The argument used by the wolf against the lamb may fairly be brought to bear on this point. Although there are dark places in the pathology of gout on which the search-light of medical investigation has not yet thrown its beams, one thing is certain—it is inherited. However temperate a man may be, he must often suffer for the gluttony and drunkenness of his ancestors, and the conditions of intellectual life, together with the strain on the nervous system which brain work necessarily causes, favour the manifestation of the inherited tendency. As examples of gout in literary men we need only mention Milton, Rochefoucauld, Gibbon, Samuel Johnson, Henry Fielding, Congreve, Walter Savage Landor, William Morris, and Sydney Smith. Among painters there are Rubens and Claude Lorraine; among actors Charles Kean. One character common to all these men, differing as they do in most other respects, is the strong sense of their writings, the robust virility of their artistic expression. Other diseases, in which uric acid is believed to play a leading part—rheumatism, gravel, and especially stone—are common in men of the most varied genius; to take the first examples that come to hand, we may mention Erasmus, Luther, Montaigne, Harvey, Leibnitz, Bossuet, Morgagni, Buffon, d'Alembert, Jean Jacques Rousseau, Voltaire, and Benjamin Franklin. Samuel Pepys hardly finds a fitting place in this galaxy, but, apart from his remarkable administrative capacity, he stands alone as a writer, and he used to celebrate each recurring anniversary of his having been cut for stone. Whether Carlyle suffered from gout or not, his "biliousness" certainly left its mark on his prophetic denunciation of shams and of contemporary mankind in general.

In strong contrast with the gouty men of genius are those tainted with tuberculosis. In these the predominant quality is a somewhat feverish excitability and tendency to mental exaltation followed by periods of mental dejection. The names of Sterne, Novalis, Keats, Stevenson, and John Addington Symonds will immediately occur to all lovers of letters; among musicians there is Chopin.

We are accustomed to look upon the manifestations of genius through the clouds of suffering as examples of mental power subduing the infirmities of the body. But it is held by some that the disease itself may be a direct stimulant to production. Dr. Charles B. Reed, of Chicago, goes so far as to say that we owe many of the literary masterpieces of the world to the action of poisons in the author's blood. In the July number of the *Forum* he says: "We are not prepared at present to insist that toxins are essential to achievement, nor that an obscure toxin will convert mediocrity into genius, but rather to suggest that the presence of such a substance in the circulation may, and does in many instances, accelerate and intensify the expression of existent intellectual tendencies." His argument is that many an author has attained eminence, "not in spite of a serious toxaemia, but possibly on account of it," the poison acting as a powerful stimulant to the brain and nervous system.

Owners of creative brains, according to Dr. Reed, are compelled to work by the toxins they elaborate, and he thinks it is not too much to say that the periods of active production probably stand in close relation to the ebb and flow of the toxic tide. Heine's genius reached its highest point during the last years of his prolonged confinement to his "mattress grave." Of the febrile affections whose toxins are stimulating, Dr. Reed selects pulmonary tuberculosis as the type, "not alone on account of its bacterial origin and the peculiar suitability of its toxins, but because its frequency and chronicity provide ample opportunities for study." He refers to the books written by John Addington Symonds while slowly dying from consumption, and quotes a letter written by him from Davos in 1882, in which he says: "If I am doomed to decline now, I can at least say that in the five years since I came here dying I have had a very wonderful Indian summer of experience. The colours of life have been even richer, my personal emotions even more glowing, my perception of intellectual points more vivid, my power over style more masterly than when I was comparatively vigorous. It seems a phase of my disease that I should grow in youth and spiritual intensity, inversely to my physical decay. It is almost pain to grasp the loveliness of the world with so much intensity when the body is so dragging."

Of Stevenson it is said that, "while prostrated by a pulmonary haemorrhage, he wrote out in three days the first draft of *Dr. Jekyll and Mr. Hyde*." During this period, says Dr. Reed, his toxic stimulation was so great and his mental systematization so complete that he neglected for hours at a time to remove the thermometer that had been placed in his mouth. His wife writes from Hyères: "After a terrible haemorrhage he fell a victim to sciatica, and was temporarily blind from ophthalmia. All light was excluded on account of his eyes, and his right arm was bandaged to his side on account of the haemorrhage. To circumvent fate he had a large board covered with paper laid across his bed, and on this on a large slate he wrote out with his left hand most of his poems in the *Child's Garden of Verses*." Under the influence of a favourable climate and life out of doors Stevenson's bodily health and vigour improved greatly. The tubercle seemed to be arrested. What, asks Dr. Reed, was the result? Necessarily, the quantity of toxin thrown out was diminished, or at least did not exceed the dosage to which he was accustomed, and he keenly felt the deprivation. Colvin says that during this year, 1894, Stevenson found himself unfit for serious imaginative writing, and a consciousness of the loss caused him many misgivings. He wrote to Charles Baxter as follows: "I am come to a dead stop. I never can remember how bad I have been before, but at any rate I am bad enough just now; I mean as to literature. In health I am well and strong. I take

it I shall be six months before I shall be heard of again." He died of apoplexy eleven months later, but Dr. Reed does not doubt that a subsequent exacerbation of the tuberculous infection would have been accompanied by a period of unusual literary activity.

We may carry the subject further. It is well known that Goethe, who lived to a splendid old age, was seriously ill in youth. Some think the affection was tuberculosis; others hold that it was syphilis. But we are not aware that any one has attributed to this disease an inspiring effect on the creative faculty of the brain. Sir Andrew Clark undoubtedly suffered from tuberculosis in early life, but his work was done after he had cured himself.

What one would like to know is under what conditions tuberculosis is an inspirer of genius. Invalidism would have the effect of enabling a man to give his mind steadily to work, even if it were but for short periods at a time. We know that Scott's infantile paralysis, according to himself, laid the foundation of his multifarious reading. A boy who cannot join in the ordinary sports of his contemporaries, if he has anything in him will be driven to develop his faculties. Stevenson has left us an inspiring example of what can be achieved in spite of almost constant bad health. The theory of stimulation by toxins is ingenious; but there must first be the creative faculty to respond to stimulation. If genius were a by-product of the tubercle bacillus, the question of the sanatorium benefit and the tuberculosis dispensary would have to be considered from a new point of view. Already eugenists in their blind enthusiasm are threatening to stamp out the germs of possible genius; it would be a serious thing if the Chancellor's well-meant efforts should complete the work of destruction in another direction.

THE URGENT NEED OF A STREET AMBULANCE SERVICE FOR LONDON.

The perils of London streets arising from the increased use of mechanically propelled vehicles has been engaging the attention of Parliament and of the Home Office. The published statistics are sufficiently alarming and fully justify the outcry which has arisen. More than 400 persons were killed by mechanically propelled vehicles within the metropolitan area in 1911, and many thousands were injured more or less seriously. Motor omnibuses were responsible for 95 fatal injuries and 1,690 non-fatal injuries. Motor tramway cars killed 27 and injured 2,459 in the twelve months of 1911. The enhanced peril of the modern motor traffic is yet further brought out when we find that in 1901–10 no fewer than 561 fatal and 22,946 non-fatal accidents were due to trams and omnibuses, against 217 fatal and 6,718 non-fatal accidents attributable to such public vehicles in the years 1891–1900, when they were mostly horse-drawn.

We do not enter into the rival claims of motor omnibuses and electric tramways, but there is one aspect of this question in which we are deeply interested and which appears to have been strangely overlooked in the public discussions which have taken place. We allude to the provision that is made, or rather not made, in the metropolitan area for dealing with the thousands of non-fatal accidents in the public streets. As long ago as 1901 this matter engaged the attention of the London County Council. They devised an excellent scheme for a rapid ambulance service, summonable by telephone to the site of the accident and equipped with comfortable automobile conveyances, with all necessary appliances to secure the prompt and easy removal of the sufferer either to hospital or to his home. They introduced clauses into a Parliamentary bill to enable the County Council to establish and maintain such a service in London. The City Corporation opposed these powers being conferred upon the Council in so far as they would extend into the area of the City Corporation. It was stated at the time that the police also opposed the County Council's proposals, and though they passed the Commons they were rejected in the Lords. The City Corporation, having control of their own police, however, at once set about establishing an ambulance service of its own. It immediately achieved a notable success, and daily the City automobile ambulances may be seen engaged in their beneficent work—even in the crowded streets the traffic makes way for them—so that the sufferer is landed in hospital within a few minutes of the receipt of the telephone call.

The rejection of the County Council's scheme was followed by the appointment of a Departmental Committee. In 1909 that Committee reported that the present mode adopted in the County of London for handling the injured was "gravely defective," and caused "much preventable detriment and suffering."

In 1909 Sir William Collins introduced and carried through Parliament a bill to enable the London County Council to establish and maintain a rapid ambulance service, summonable by telephone, similar to that which had proved so successful in the City. We are, however, unable to learn that any practical step has been even yet taken to put this Act into operation, notwithstanding the increasing number of accidents in our streets, and the immense amount of suffering occasioned by the present haphazard and gravely defective methods which are employed. Last month the Chairman of the General Purposes Committee, when interrogated on the subject, replied that that Committee was "making exhaustive inquiries, and when its negotiations were concluded it would report to the Council." We are not surprised that this callous reply was received with ironical cheers or jeers. No further inquiry is needed; the Departmental Committee inquired for two years. The case for a modern adequate ambulance service for London has been amply made out. The London County Council is the only public authority which is entrusted by statute with providing such service. The precise cost of maintaining an ambulance station is known as the result of the City's experience. We have heard of offers made to the County Council by one or two hospitals of sites for such stations, and we have been informed that the use of an up-to-date ambulance has also been offered to it. A relatively small expenditure for maintenance annually would remove a reproach which is now amounting to a scandal. We recently heard of a case of a woman with a compound fracture of the leg, due to a motor omnibus accident, being removed to hospital on a hand barrow. It is idle for the County Council to endeavour to thrust its responsibility on to other authorities. No other local authority in London can attempt the work without being liable to be surcharged. It is the County Council's responsibility alone. At the election of that body nearly three years ago a majority of its members were pledged to put the powers of the Metropolitan Ambulances Act (1909) into operation. Their tenure of office terminates next March. Unless the present Council speedily redeems its pledge it will be justly accused of having abused its trust.

Meanwhile the Home Secretary's replies in the House of Commons are far from satisfactory. The need for additional public control over motor omnibuses, their number and routes, may call for some investigation as well as the question of contribution to the repair of the roadways they destroy; but as regards the provision of a street ambulance service, not investigation but action is needed, and the proposed Select Committee ought not to be made an excuse for further delay. If the Home Office and the County Council would cordially co-operate, an adequate service might be in working order before the close of the year.

TUBERCULIN IN AUSTRALIA.

A FEW years after the death of Mr. James Brown, twenty years ago, there was established in his name a memorial trust, and one outcome was the foundation of a sanatorium at Kalyra, South Australia. It is under the general medical supervision of Dr. Charles Reissmann, who is now on a visit to Europe with a commission from the Commonwealth Government to inquire into and report on the systems of dealing with tuberculosis in Great Britain and Europe. He informs us that the Australian quarantine rule in regard to the landing of consumptives is no longer a dead letter. Any passenger deemed to be suffering from tuberculosis, even in an early stage, is usually absolutely excluded; at the best he is allowed to land only on execution of a heavy bond, which binds himself to leave Australia if not pronounced to be recovered at the end of six months. This rule is applied impartially, and includes first-class passengers. It is well that the fact should be generally known, since, although it is much less common to send consumptive patients to Australia than formerly, it appears to be done much more often than the experience of individual medical men on this side might perhaps suggest. The latest report relating to the Kalyra sanatorium in question shows that its administration has certain features of special interest. On the discharge of a patient a notification is sent on a special form to the medical man on whose recommendation he was admitted. It gives the date of the discharge of the patient, his address, the results of treatment at the sanatorium, and full details as to his condition on the day he left, including a note as to the dose of tuberculin which he last received. Another form is sent at regular intervals to every patient after his discharge, requesting him to fill in certain details as to the existing condition of his health and ability to work. A third form is a diagram representing in one figure both sides of the chest, as well as the back and front, thus enabling any lateral lesions that may exist to be graphically displayed. Altogether the charts and tables in use in the sanatorium number as many as thirty-four, certain of them showing the present condition of all patients discharged during each of the past six or seven years. In the year 1905–6, of 39 patients classed as in Stage I (Turban) 12 are dead, 11 cannot be traced, and 16 are alive. Of 51 classed as in Stage II, 35 are dead, 8 cannot be traced, and 8 are alive. Of 47 classed as in Stage III, 40 are dead, 5 cannot be traced, and 2 are alive. In other words, of patients in all stages discharged from the institution between six and seven years ago, 38 per cent. are still alive. It is known that a large proportion of the patients who cannot be traced have left South Australia, and that the others are probably still living, since no record of their death can be discovered. The systematic employment of tuberculin as a curative agent was commenced at Kalyra in 1909–10. In the earlier years the percentage of patients at all stages discharged "apparently cured" varied from 13.6 to 26.7; in the later years the corresponding percentage varied from 28.6 to 35.9. In order to show that this difference was not due to more careful selection of patients for admission, those classified on admission as being in Stage I were separately considered. In the three years before the introduction of tuberculin treatment the percentage of those cases discharged as "apparently cured" was 62.1, and in the three subsequent years 78.7. While these figures are held to disprove selection, they do not prove absolutely, it is pointed out, that it is to the introduction of tuberculin treatment that the improvement in the later years is due. Even among cases properly classed as first-stage cases there are considerable differences, individuals varying in recuperative power, and different strains of bacilli also varying in virulence. Nevertheless, Dr. Reissmann thinks that the evidence justifies the conclusion that tuberculin as now used at the sanatorium is of value and that its administration should be continued. The tuberculin used is a mixture of T.R. and T.O.A., the reason for the admixture being that T.R. is an endotoxin, whereas T.O.A. is an exotoxin, and, since in pulmonary tuberculosis each of these poisons is found, it is thought advisable to attempt to create active immunity against both of them. Fresh dilutions from concentrated solutions are made weekly; the dose administered varies from a mixture containing 0.0000025 mg. T.R. and 0.000125 mg. T.O.A. to a maximum in a very few cases of 1 mg. T.R. and 50 mg. T.O.A. The dose on each occasion is determined by careful consideration of the existing state of the patient and of his previous record. Should a definite temperature reaction of 1° F. follow the administration of a dose, and should a similar rise occur when the same dose is given after an interval of eight to ten days, the treatment is discontinued. The treatment otherwise consists of rest and graduated exercise at stated intervals, together with inhalation of various vapours when they seem to be required. The death-rate in South Australia from pulmonary tuberculosis has been falling for over two decades. In 1911 it was only 0.68 per 1,000, and the mean for the five years ending the beginning of this year was only 0.81, whereas in England the mean rate for the five years ending 1909 was 1.16 per 1,000—a much higher figure than in South Australia, but a much lower one than in nearly any European country.

THE PARASITE OF CANCER.

HISTORY repeats itself with a somewhat monotonous regularity in science as in other departments of human activity. This is particularly the case in regard to cancer. Some say that there is no parasite of cancer, others declare with equal confidence that they have seen it. The shades of the countless parasites which have been "laid" long since were so real and so obvious in their time that it is no use telling the protozoological Scrooge that they were but phantoms; for him they existed. We have now to announce a new parasite—if it is not one of the old spectres resurrected—and to discuss it, without being able to say whether this time the problem which has baffled so many has at last been solved. Dr. Robert Behla,[1] Director of the Medical Department of the Royal Prussian Statistical Office in Berlin, dogmatically asserts that he has succeeded in cultivating a protist (*Blastozoon globosum*), which he holds to be the true cause of cancer. The article is written in a tone of assurance which does not inspire confidence, and is overloaded with technical detail to such an extent that the true object of the discussion is hidden from view. The fundamental claims and arguments might have been set forth in two pages instead of thirteen, and the strength of the latter would have gained considerably thereby. On one point we can agree with the author without hesitation—namely, that his experiments must be carefully and thoroughly controlled by capable workers. The results of these control

[1] *Deutsche Medizinalzeitung*, October 19th, 1912.

experiments must, however, be set forth in a very different manner to that adopted by Dr. Behla. Brushing aside a mass of *a priori* argument in favour of the existence of a parasite, we come to alleged facts. Certain appearances in new growths have for a long time been recognized. At first they were regarded as evidence of the existence of parasitic bodies; later, the view generally accepted was that they were cell inclusions. It is not quite clear to which appearances Behla refers, since he states that the so-called parasites of Schmidt, Doyen, Plimmer, Leopold, Jaboulay, and others, are not identical with his. At any rate he has succeeded in cultivating these round bodies in excised portions of the periphery of non-ulcerating new growths, to which he adds a fluid consisting of 100 grams of extract prepared by allowing distilled water to act on raisins (sultanas), in the proportion of 1 part of raisins to 10 of water, for twelve hours, and 5 grams of grape sugar filtered and sterilized. The piece of growth is placed in the warm fluid directly from the body—that is, before it has time to cool down—and is incubated at once. The growth is tested after three days. Subcultures can be made on gelatine or agar, etc., acidified with tartaric acid, but the bodies will not grow on these mediums directly. The statement made is that the same parasites are obtainable from the majority of malignant growths, but not from all and not from benign growths. It is said that the parasites are neoplastic, which must mean that Dr. Behla has injected the cultures into suitable animals and has obtained typical malignant growths, but such experiments are not detailed. He also says, again without detailing how the tests were carried out, that the serum of persons suffering from cancer agglutinates these cultures. He has found the protists in various organs affected, but not in the blood. Other statements contained in the paper might be adduced, such as the view of the author that the parasites belong to the protists, and not to the protozoa, and so forth, but these details need not concern us at present. What we wish to bring out is that a well known careful observer professes to have cultivated certain parasites from malignant growths and from malignant growths alone; that these parasites are supposed to be but one stage in a complicated life-cycle of a protist, and that certain evidence is adduced in support of the contention that these bodies are the living cause of carcinoma, sarcoma, and the other diseases usually included under the term "cancer." The work will be repeated by those who are skilled in sterile working, and, as the author points out, in three months the question might be settled. In the meantime we await further evidence.

THE ODOUR OF SANCTITY.

THE phrase "odour of sanctity" is often heard, but few, perhaps, have ever asked themselves what it means, or whether it has any meaning at all. Scoffers have applied it in a literal sense, associating it with the fact that many saints have regarded washing as a sinful indulgence; to them, as to the demons in the *Dream of Gerontius*, the word "saint" connotes a bag of bones—

> That rattle and stink
> E'en in the flesh.

The phrase has now come to be used in a mystical sense. Yet it is unquestionable that it was in a former day often intended to be taken literally, as indicating a perfume of holiness, which might be regarded as a counterblast to the sulphurous fumes generally associated with the evil one. Sometimes, no doubt, the odour of sanctity is nothing more than a trick to delude the credulous, as in the case of a Maronite nun of whom Renan heard in Syria. She was considered a saint mainly, it would appear, on the ground that a smell of incense came from her, and pervaded her cell. After her death the convent was pulled down. During the work of destruction pipes were found in the wall, and through these came the fumes of incense. This discovery caused great scandal. Renan characteristically said: "We really are too merciless in disclosing the pipes which carried the incense fumes. Sometimes I say to myself that it would have been better not to touch them, and to leave the worshippers in their edifying belief." In Mallory's history of Prince Arthur he says that when Sir Launcelot's companions found him dead they noticed "the sweetest savour about him that ever they smelled." The odour of sanctity is a real thing, but there is nothing miraculous about it. Some thirty years ago Dr. W. A. Hammond, in a paper read before the American Neurological Association, called attention to the fact that the odour of sanctity ascribed to the saints by Goerres had a neuropathic explanation. He pointed out[1] that odours were emitted during conditions of hysterical excitement, of sexual excitement, and of religious exaltation. Sometimes the odour may have been due to disease. In patients who have long suffered from suppurative conditions it has been observed that an aromatic odour somewhat resembling that of strawberries is produced in consequence of the invasion of the pus by the *Bacillus pyocyaneus*. Before the antiseptic era blue pus was often observed in wounded persons, and its characteristic odour was well known. Again, patients who have been treated with turpentine diffuse an odour of violets, and that their urine has this odour is a common observation. Dr. George Dumas, of Paris, some time ago made a critical investigation of a number of cases in which mention is made of the odour of sanctity in the lives of certain saints, and he supplies physical explanations of them. The odour varies, being compared to the smell of the lily, the rose, the violet, the pineapple, and so forth. Subject to special modifications, Dumas gives the following general formula for the odour of sanctity: $C_6 H_{12} O_3$. He explains the odour of sanctity in the case of Saint Teresa by diabetic acetonaemia. It is well known that the breath of diabetics has a sweet smell. Dr. Dumas's theories are plausible, but the most interesting thing to us about his essay is that he accepts as facts statements which used to be dismissed as fables, and seeks for rational explanation of them. This is the true spirit of the scientific inquirer. But the explanation is, after all, only a hypothesis, for there is not, as far as we are aware, any kind of proof that the noble woman whom that Philistine of genius, Macaulay, called the "mad nun of Avila," actually suffered from diabetes. Something akin to the odour of sanctity has, it is said, been perceived at spiritualistic séances, and was supposed to be of psychic origin. This gave rise to a good deal of scoffing, but, as has been said, it is quite possible that perfumes accompanied the manifestations, and there is no need to have recourse to the spirits for their production. It may be added that scientific testimony to the possible reality of the halo of sanctity has also been adduced. Féré[2] believed that he witnessed the phenomenon in the case of two women, victims of migraine, in whom during an attack there suddenly appeared around the head an orange-coloured light stretching out from it for 20 centimetres. At the same time the skin had an orange tint. In one of these patients the phenomenon lasted several hours, in the other but a few minutes. He refers to the case of a third woman who was often suddenly awakened by a feeling of anxiety; on these occasions, he says, a halo was visible about her head. It may be added that phosphorescent sweating has been seen in certain rare cases after eating phosphorescent fish, or even as an idiopathic phenomenon. An instance in which the body linen became luminous after extraordinary exertion is on record. The phosphorescence is believed to be due to bacilli.[3] These facts

[1] *Neurological Contributions*, vol. i, 1881.
[2] *Revue de médecine*, No. 4, 1905.
[3] Malcolm Morris, *Diseases of the Skin*, p. 557.

may help to explain statements made by hagiographers and certain appearances sworn to in trials of alleged witches.

THE HOME OFFICE AND FLANNELETTE.

SOME fifteen or twenty years ago an ingenious individual first contrived to deal with cotton in such a way as to produce a fabric resembling one of pure wool at a fraction of its cost. The next step—doubtless taken not by an inventor, but by a salesman—was to dub the material "flannelette," and to suggest to the public that it was in all respects quite as desirable a material for undergarments as flannel itself. Flannelette in appearance resembles real flannel, possesses some of the advantages of the latter—namely, it is soft and warm to the feel—and, owing to its initial fluffiness of surface, retains a good deal of body heat. But when it has been washed several times flannelette soon loses its warmth-retaining powers, and from the beginning most makes are highly inflammable. The latter fact only became known to the general public when deaths due to the use of flannelette became a standing item on lists of inquests and when this JOURNAL and others had repeatedly drawn attention to the dangers of this substance. Furthermore, recognition of this new danger did not lessen the annual mortality from injuries by burning, especially in children, for so soon as the manufacturers became aware that the sale of flannelette was imperilled they countered by adding to their trade description of the article terms suggesting that their own particular product was uninflammable and therefore devoid of risk. There are methods by which flannelette can be rendered uninflammable, but only one or two firms have even attempted to apply them. Hence the deaths continued. That was the position some three or four years ago, and it remains almost unaltered at the present time, the annual roll of deaths being practically as high as ever. Nearly all the flannelettes on the market burn almost as rapidly as cotton-wool, and with one or two possible exceptions those specifically described as non-inflammable are little better. This is not a case to which the maxim *Caveat emptor* can properly be applied, the vast majority of the purchasers of flannelette being quite poor people who are at once tempted by the price and deceived by the terms under which it is sold. It is the Home Office which should intervene in the matter, and it practically admitted its responsiblity a year or more ago by appointing a Departmental Committee to consider the question of deaths from burning brought about by flannelette. Since there was no practical result therefrom, it was recently approached by a deputation from the National League for Physical Education and Improvement, which for two or three years has been active in the matter. The case for intervention was ably set out by Professor Howard Marsh, Sir Lauder Brunton, and Mr. T. C. Taylor, M.P., the latter indicating that the desire was that if it were impossible to prevent altogether the sale of inflammable makes of flannelette, false descriptions of its quality in regard to inflammability should be rendered definitely illegal. The reply made on behalf of the Home Office was to the effect that the Home Secretary was fully aware that the public was being deceived in the matter, but he was advised that it was impossible to deal with the question by an amendment of the Merchandise Marks Act, that there would be considerable difficulty in setting up a standard by which the non-inflammability of any given product could be tested, but that any suggestions on the subject would be considered. This was indeed but cold comfort; the Home Secretary has already had the suggestions of his own Committee, but has done nothing, and further suggestions from outside bodies seem likely to find their way to the pigeon-hole. No doubt there are difficulties, but with such difficulties it is the duty of the Home Office to deal, so the reply made would seem to be a confession of weakness and utility.

THE ROYAL MEDICAL BENEVOLENT FUND GUILD.

THE annual meeting of the Royal Medical Benevolent Fund Guild, whose appeal is largely to the wives and daughters of medical men for their help in money and service, took place at the Mansion House on Friday, November 1st, with the Lady Mayoress in the chair. Lady Tweedy, the president of the Guild, in presenting the annual report, said that in order to encourage self-reliance it had been decided to grant loans, in certain cases, either with or without interest according to circumstances. She added that the work of the Guild was growing in every direction, but the increased membership hardly kept pace with the appeals for help, and visitors were badly needed. From every centre came the same complaint, that many subscriptions had either been reduced or refused altogether, owing to the increase in taxation and in the cost of living; in consequence the fund lacked the means to respond to a number of applications. During the past year it had been compelled to refuse help in several cases, whilst in others the amount given had been less than usual. Despite the decrease in subscriptions, however, the treasurer (Mrs. Scharlieb, M.D.) was able to report that owing to the generosity of friends the Guild had done well financially, the receipts amounting to £1,625 as against £900 last year. Stepney, which was not usually regarded as a rich district, had sent £50, the proceeds of a concert; and an annual income of £100 had been promised, under certain conditions, by a lady who took a deep interest in the work of the Guild. Thanks to the improvement in its finances the Guild had been enabled to purchase a ten years' annuity of £100 a year; but funds were still urgently needed to meet the inevitable expenses incurred in carrying on the work. An interesting account of the work of the visiting committee was then given by Mrs. Liveing; and another speaker suggested the extension of the good offices of the Fund to struggling doctors who were endeavouring to bring up and educate their children on pitiably inadequate incomes. The meeting concluded with a special appeal for gifts and subscriptions towards the Father Christmas branch of the Guild.

MEMORIAL TO ADMIRAL SIR FREDERICK WILLIAM RICHARDS.

THE brother officers and friends of the late Admiral of the Fleet Sir Frederick Richards having expressed a strong desire to honour his memory, a meeting will be held at the Royal United Service Institution, Whitehall, London, on Wednesday next, November 13th, at 12.30 o'clock, when the project of instituting a memorial will be discussed and an executive committee will be appointed. As the work of Sir Frederick Richards, essentially patriotic in intention and in effect, was of inestimable, if unobtrusive, benefit to his country and remains a high example of devotion to duty in the Royal Navy, it is hoped that the meeting, which is open to all, may be representative, not only of the navy and the army, but of the nation at large. Sir Frederick Richards was a broad-minded, warm-hearted man, and ever a very good friend of the Royal Naval Medical Service. His memory is venerated by many officers of that service, who will, without doubt, be glad to take their share in the proposed memorial.

THE British Association has appointed a committee on colour vision and colour blindness. The committee consists of Professor E. H. Starling (chairman), Professors F. Gotch, Leonard Hill, A. W. Porter, and A. D. Waller, with Dr. Edridge-Green as secretary.

AT the meeting of the Surgical Section of the Royal Society of Medicine, to be held on Tuesday next, November 12th, at 5.30 p.m., there will be a discussion on sarcomata and myelomata of the long bones. Sir Alfred

Pearce Gould will deal with the clinical diagnosis, Sir John Bland-Sutton with the pathology, Mr. G. E. Gask with prognosis, Mr. A. Reid with *x*-ray appearances, and Sir Frederic Eve with treatment. Among other speakers will be Mr. R. C. Elmslie, Mr. Ernest Shaw, Mr. C. R. C. Lyster, Mr. Gwynne Williams, Mr. Ironside Bruce, and Mr. S. G. Scott.

Medical Notes in Parliament.

National Insurance Scheme.

Sanatorium Benefit.

Mr. Fell asked on October 30th if only one-fifth of the consumptives in this country would be entitled to sanatorium or any other benefits under the National Insurance Act; and if the Secretary to the Treasury could give any estimate of the amount of the charges which would fall on the local authorities in respect of the consumptives who would not be entitled to benefits under the National Insurance Act, but who would have to be placed in sanatoriums by the local authorities under another Act. Mr. Masterman replied that only insured persons or dependants of such persons could be treated at the expense of the National Insurance Fund. The number of these would bear a continually increasing proportion to the total number of consumptives, and it was probable that they would ultimately form a large majority of the consumptive persons in the country. The Insurance Act also contained provisions whereby sanatoriums and other institutions for the treatment of tuberculosis might be made available for non-insured persons. He was unable, however, at present to give an estimate of the cost of the treatment of the consumptives who were not insured persons or dependants of insured persons.

In reply to a further question by Mr. Fell, Mr. Masterman said he should not think that more than one-fifth of the people at present suffering from consumption were employed persons.

On October 31st Captain Gilmour asked whether the Insurance Commissioners had made any regulations as to the terms under which patients would be admitted to sanatoriums for treatment; and if his attention had been drawn to the fact that in some cases the patient was required to provide an outfit of clothing to the value of £5. Mr. Masterman said that no regulations had been made by the Insurance Commissioners as to the terms under which patients would be admitted to sanatoriums for treatment. Such terms were primarily a matter for agreement between the Insurance Committee and the managers of the sanatorium, but the arrangements made required the approval of the Commissioners, and they would not approve of any agreement which would impose upon an insured person the liability mentioned in the question.

Sir Edward Beauchamp asked whether the Insurance Committee of a county had power to extend sanatorium treatment to dependants of insured persons resident in the county; and whether any Insurance Committees had made the necessary arrangements for the treatment of such persons. Mr. Masterman replied that the answer to the first part of the question was in the affirmative. Some Insurance Committees had already made arrangements for treatment of dependants, and the Commissioners proposed to issue a circular letter recommending that other Insurance Committees should consider the question of extending sanatorium benefit to the dependants of insured persons.

Mr. Fell inquiried if that would mean an increased charge. Mr. Masterman replied that there was sufficient money already available.

Mr. Guiney, on November 1st, asked the Chancellor of the Exchequer what reasons were adduced for sending tuberculous patients from Munster to the Newcastle sanatorium, county Wicklow, when there was accommodation available in Munster at the Heatherside sanatorium, Doneraile, and the charge reasonable; and where were the hospitals available in Munster for the treatment of chronic tuberculous patients situate. Mr. Masterman, in reply, said the local Insurance Committees were responsible for making the most suitable arrangements for each person entitled to sanatorium benefit. They were not restricted to making arrangements with institutions in their own area, and the decision as to where a particular individual should be sent necessarily depended upon several considerations, including the cost of travelling and of maintenance, and the character of the treatment required. No institution in Munster had yet been approved specifically for the treatment of advanced cases of tuberculosis.

In reply to Mr. Fleming, Mr. Masterman said that his attention had been called to a resolution passed by the Reading Insurance Committee with reference to existing accommodation for cases of tuberculosis, and he understood that the Insurance Commissioners had supplied the committee with a list of approved sanatoriums to which insured persons suffering from tuberculosis might be admitted. The complete separation of Poor Law institutions from sanatorium treatment under the Act was a principle definitely embodied in the bill in its passage through this House, and he had no reason to believe that the House had any desire to reverse that decision.

On November 4th Mr. Remnant asked the Chancellor of the Exchequer with regard to a man at Barking who, after making several applications to the Essex Insurance Committee for sanatorium benefit, had been compelled to obtain the benefit elsewhere, and inquired whether the Essex Insurance Committee would be directed to refund the cost. Mr. Masterman replied that he was informed by the Essex Insurance Committee that at the time of the application referred to they found that the attitude of the local medical profession rendered it impossible for them to obtain the necessary medical report on the applicant's physical condition. Consideration of the application was consequently deferred until October, and when the case came up at the first meeting of the committee in that month it was found that the applicant had made a private arrangement for treatment in a sanatorium. He was communicating further with the Insurance Committee with regard to the case.

Mr. Touche asked the Chancellor of the Exchequer on November 5th if he was aware that, according to the information available at the Local Government Board, over 2,300 persons of sixteen years of age and upwards, and over 750 under sixteen years of age, suffering from tuberculosis, were being treated in the Poor Law infirmaries and Metropolitan Asylums Board's institutions, many of whom presumably would be entitled, under the National Insurance Act, to sanatorium benefit; that the cost of the treatment of these patients involved a very large expenditure, which was at present a charge upon the poor rates of the metropolis; and could he say what provision out of the National Health Insurance funds it was intended to make for those cases. Mr. Masterman in reply said that the National Insurance Fund could only be charged with the cost of treating insured persons and the dependents of insured persons, but the National Insurance Act and the Finance Act, 1911, had made available a sum of £1,500,000 to aid the provision of sanatoriums and other institutions in which both insured and non-insured persons might receive treatment.

Mr. Fell asked if certain definite sums had been assigned for the payment of the sanatorium benefits under the National Insurance Act; and if those sums formed a part of the general fund and would be available for the general expenses of the Act. Mr. Masterman replied that definite sums were allocated under Section 16 (2) of the National Insurance Act for the provision of sanatorium benefit. Those sums were not available for other purposes of the Act. The Act definitely allocated certain sums that must be devoted to sanatorium benefit.

On November 5th Captain Clay asked whether the National Insurance Commissioners had power to pay rent for an insured person who had been ordered domiciliary treatment for tuberculosis and who was destitute owing to his inability to earn a living? Mr. Masterman replied No. He was advised that the Commissioners had no power to authorize payments of insured persons' rent from the funds available for sanatorium benefit under the National Insurance Act. Sickness benefit of 10s. a week (or 7s. 6d. for women) would, however, be payable after the six months' waiting period, under the ordinary conditions, to insured persons who were receiving domiciliary treatment in their own homes.

In reply to Mr. Pollock, Mr. Masterman stated that the numbers of persons who died through tuberculosis in 1910 in England and Wales, in Scotland, and in Ireland were 51,317, 8,517, and 10,016 respectively. No figures were

available as to the number of persons suffering at the present time, and no estimate could be formed except on the basis of an assumed ratio between deaths and cases, which was necessarily speculative and must vary in the different forms of tuberculosis. The proportion of tuberculous persons who might obtain treatment under the Act would, if insurance committees extended the benefit to dependants, might ultimately exceed 50 per cent.; but as only those who had been at work at some time subsequent to July 15th could be insured, the present proportion must be, and remain for a time, very much lower.

Mileage Allowance.

Mr. Charles Bathurst, on November 1st, asked whether, in the revised system of payment of the doctors for their services under the National Insurance Act, any difference would be made in the mileage allowance for urban and country practitioners respectively. Mr. Masterman replied that no part of the new grant which Parliament would be asked to make was specifically ear-marked for mileage, but that did not prevent an arrangement for this purpose being made. The distribution of the total sum to be provided for medical remuneration, and the assignment of special portions to special services, was largely a matter for the medical profession, and any proposals which appeared to command the general support of the profession would receive careful consideration.

Medical Benefit.

Mr. Laurence Hardy asked the Chancellor of the Exchequer on November 4th whether, when he laid down as one of the conditions for the medical service in connexion with the National Insurance Act that the practitioners should in future resort to modern means of exact diagnosis, he intended to include under this head all the expense of examination of blood, sputum, etc., and the cost of vaccine treatment, within the ordinary allowance granted to the medical profession; whether under the term "medical appliances" such articles as trusses and other appliances of a similar character were included, and would they be paid for out of the extra 6d. allocated for unusual expenditure; and could he state what was the authority which was to decide whether a doctor had or had not resorted to the most modern means of diagnosis. Mr. Masterman, in reply, said that it was generally recognized, by none more than the medical profession themselves, that the standard of contract practice up to the present had been on the whole far from satisfactory. Now that the State was providing a very substantial increase in the remuneration of the medical profession, it was proposed that the standard should also be improved, and it was contemplated that, in consideration of this increased remuneration, the practitioner on the panel would provide medical treatment for the benefit of the insured persons to the same extent and on the same lines that he would for his patients in his ordinary private practice. Trusses were not included in the list of appliances which was scheduled to the Regulations, and therefore would not be paid for from the amount allowed for drugs and appliances. It would be for the Commissioners to decide whether a proper standard of diagnosis and treatment was being maintained by the medical practitioners who undertook the attendance and treatment of insured persons. It would not be practicable at present to foreshadow in detail the methods which the Commissioners might from time to time find it advisable to adopt in order to satisfy themselves in this respect. He might say, however, speaking broadly, that it was intended to rely at the outset to a great extent on the co-operation of the local medical committees and the medical profession generally to secure the object in view.

Mr. C. Bathurst asked whether, in the revised system of payment of the doctors for their services under the National Insurance Act, any difference would be made in the mileage allowance for urban and country practitioners respectively. Mr. Masterman said, in reply, that no part of the new grant which Parliament would be asked to make was specifically earmarked for mileage, but that did not prevent an arrangement for this purpose being made. The distribution of the total sum to be provided for medical remuneration, and the assignment of special portions to special services, was largely a matter for the medical profession, and any proposals which appeared to command the general support of the profession would receive careful consideration.

Mr. C. Bathurst asked whether it was realized that the expenses under that head of the rural practitioners were considerably more than those of the urban practitioners? Mr. Masterman replied that was so, but rural practitioners would have several compensating advantages in other arrangements, such as the provision of drugs.

Sir H. Craik asked if the remuneration left to the doctors would depend upon the largeness or the smallness of the amount they required for mileage? Mr. Masterman said no special payment for mileage would be included in the total amount, amounting, he thought, to something like 9s. per head for doctors and drugs. Sir H. Craik said that the doctors would lose as the mileage increased. Mr. Masterman replied that the doctors would get the mileage allowance.

Mr. Falconer asked whether any allowance for mileage to country doctors would necessarily come off the allowance to the town doctors? Mr. Masterman replied not necessarily; it all depended upon what representations were made by the medical profession as to how best to distribute the money.

Mr. Lloyd asked whether the doctors would not lose if the mileage increased? Mr. Masterman said that the mileage would be paid to the doctors.

Sir G. Younger: Out of the General Fund?

Tuberculous Sailors and Soldiers.

Mr. Astor asked the Secretary of State for War how many beds in sanatoriums, as distinguished from beds in hospitals, military or otherwise, were available for the reception of insured persons in His Majesty's forces suffering from tuberculosis. Colonel Seely answered that soldiers who developed tuberculosis were discharged from the army and then became entitled to sanatorium benefit. The method by which treatment in sanatorium could best be ensured for them was at the moment being investigated.

Mr. Astor also asked a similar question with regard to insured persons in His Majesty's navy suffering from tuberculosis, and Dr. Macnamara replied that there was no special sanatorium accommodation set apart for the persons referred to. Patients were treated in the ordinary hospitals.

Sanatorium Grant (Ireland).

Mr. Hugh Barrie asked the Chief Secretary for Ireland what was the share of the county of Londonerry, on a population basis, of the £145,000 given to Ireland for sanatorium purposes, and what would be its proportion of the sum remaining since the grant of £25,000 was made to the Women's Health Association for the erection of a sanatorium at Peamount, co. Dublin. Mr. Birrell said that if the whole £145,000 had been distributable between county areas in proportion to population, the share of the county of Londonderry would have been approximately £3,300. The share of the county in the sum of £120,000, which was available for immediate distribution on population basis, was £2,729. As regards the balance of £25,000, he referred the hon. member to the reply given to the question asked on the subject by the hon. and learned member for North-East Cork on October 17th (BRITISH MEDICAL JOURNAL, October 26th, p. 1159).

Housing in Scotland (Royal Commission).—In reply to Mr. Munro-Ferguson, Mr. McKinnon Wood said that he was satisfied that the terms of reference to the Housing Committee included inquiry into urban and rural housing generally.

Experiments on Living Animals.—Mr. Lansbury asked the Home Secretary if the formation of his new advisory body to assist him in the work of granting licences and certificates for experiments on living animals, as recommended by the Commissioners in their report of last March, was now completed; and, if so, whether the names of the gentlemen composing that advisory body could be made public. Mr. McKenna said that he had not yet been able to complete the arrangements, but hoped that the matter might be settled before long.

THE recently formed Municipal Waterworks Association held its first meeting in London a short time ago. It already has 100 members, representing 21 municipal authorities of over 50,000 population concerned with the water supply of 16,000,000 people. These and other facts were brought out at a luncheon given by Mr. E. B. Barnard, Chairman of the Metropolitan Water Board.

England and Wales.

[FROM OUR SPECIAL CORRESPONDENTS.]

LONDON.

Treatment of Tuberculosis.

With reference to the letter from the Local Government Board on the subject of the arrangements to be made in London for the administration of sanatorium benefit,[1] the Public Health Committee, in a report to the London County Council on November 5th, admitted the need for a central organizing authority in each county, owing to the number and distribution of municipal, charitable, educational, and other institutions which would form part of the machinery for dealing with tuberculosis. The scheme of supervision outlined in the report of the Departmental Committee on Tuberculosis centred upon the county councils, and there was nothing in that report inconsistent with the view that it was contemplated that in London, as in other parts of the country, the authority to be entrusted with the organizing and supervisory duties should be the County Council. This was consonant with the special position held by the County Council in London with regard to public health. If this were the intention of the Local Government Board it was essential that the point should be made quite clear and the necessary authority provided. The Public Health Committee felt that the Council would not desire the settlement of this question to interfere with the provision at the earliest possible date of institutional treatment for persons in the county suffering from tuberculosis, and the committee proposed, therefore, without prejudice to any future action, to make preliminary inquiries as to the provision of the necessary temporary accommodation. Meanwhile, the committee recommended that the Local Government Board be asked whether it intended to place the London County Council in the same position as other county councils in regard to tuberculosis treatment.

Commenting on this proposal, the Finance Committee remarked that the letter from the Local Government Board gave rise to many questions which would involve considerations of great financial importance to the Council. As to the suggestion that the Public Health Committee should communicate with the Metropolitan Asylums Board and other authorities, the Finance Committee was of opinion that in the present uncertain state of affairs nothing should be done which would in any way commit the Council to any particular course of action with regard to making either permanent or temporary arrangements.

At the meeting of the managers of the Metropolitan Asylum District on November 2nd a letter was read from the Local Government Board, informing the managers that the Government had come to the conclusion that it would be impracticable to obtain the necessary legislation in the near future to remove the obstacles to the managers providing the sanatorium accommodation required for London for the purposes of the National Insurance Act. The Board stated that in the circumstances it was not possible to adopt the suggestion of the Departmental Committee on Tuberculosis that the managers of the Metropolitan Asylum District should provide some of the institutions required. The recommendations of the Departmental Committee could best be met by some friendly arrangement between the managers and the London County Council, and the Local Government Board had written to the Council to this effect.

The letter was referred to the Law and Parliamentary Committee pending the receipt of a communication from the London County Council.

Medical Control of Farmfield Inebriate Reformatory.

The Public Control Committee stated that, having regard to the fact that a very large percentage of the women admitted to the Farmfield Inebriate Reformatory had proved quite unsuited for detention there, and that many cases were of a very troublesome and refractory nature, the question had arisen whether the existing arrangements under which the reformatory was managed were the most advantageous. At present there was a lady superintendent who was the principal officer at the institution, and the medical side of the work was performed by a visiting medical practitioner. The duties attaching to the position of superintendent of such an institution were onerous and exacting, and the committee was of opinion that the responsibilities were such as could not reasonably be expected to be undertaken successfully by a woman. The alternative was that a fully qualified medical man should be placed in charge of the reformatory, having regard especially to the fact that during the last few years the complexion of the work there had undergone a great change. The value of the appointment of superintendent was approximately £200 a year, and the present medical officer received a salary of £120 a year, out of which he had to provide medicines. The committee's proposal was not likely to cause an increased expenditure, as, in its opinion, it would be possible to obtain a suitable medical man for the combined positions at a salary of £300 a year, with fire, light, and residence.

On the recommendation before the Council:

That the Council do approve the principle that the officer in charge of the Farmfield Reformatory shall be a fully qualified medical man, and that the Public Control Committee be authorized to take the necessary preliminary steps to give effect to the resolution,

some discussion arose as to the best method of securing the efficient management of the institution. An amendment was moved that the words "or woman" be inserted after the words "fully qualified medical men."

Mr. M. Tasker said he must protest against the proposal to ask a man with full medical qualifications to accept this position at a salary of a paltry £300 a year. The Council required the services of a man who had specialized in a particularly disagreeable and difficult branch of work. The proposed salary was about half the sum the Council should offer.

The Rev. Dr. Scott Lidgett thought that the institution should be controlled by a medical man, a competent woman being appointed to the principal staff. In work of this special nature the sympathetic influence of a woman was of even more importance than medical treatment. It would be an advantage, perhaps, to secure the services of a woman with a medical qualification. The influence of a matron must be inferior to that of a woman who was capable of undertaking the scientific medical treatment of this terrible complaint. The wisest course would be not to prejudge the question by barring out all possible women candidates, but to decide after the replies to the advertisement had been received.

Mr. W. Haydon, Chairman of the Public Control Committee, mentioned that one of the duties that fell to the officer in charge of the reformatory was the superintendence of a farm of 400 acres attached to the institution. He agreed that the sum suggested by the committee as a suitable salary was too low, but he believed that for the successful management of the institution there must be a medical superintendent.

Mr. Yeo thought the proposed salary ridiculous; it should be at least £600.

In reply to a question, the Chairman of the Committee said the appointment was intended to be a whole-time one.

Mr. Edward Smith asked if it was intended to put a medical man in charge of a 400-acre farm; the institution appeared to require a steward and a matron as well as a medical superintendent. He urged that the report should be taken back and a complete statement made.

A question then arose as to whether or not board was to be included in the emoluments of the position, and, after Mr. T. Hunter had expressed the opinion that a whole-time medical superintendent was not needed for an institution containing only 100 patients, the Chairman of the Committee took back the report, promising to submit a more exhaustive review of the working of the establishment.

The Medical Treatment of London School Children.

Arrangements for the Year 1912–13.

The Education Committee of the London County Council on November 6th had before it a long report by the Children's Care Subcommittee as to the arrange-

[1] British Medical Journal, October 26th, p. 1165.

ments to be made during 1912–13 for the medical treatment of London elementary school children.

The subcommittee proposed that provision should be made during the year ending July 31st, 1913, for the treatment of 48,613 defects, or, omitting minor ailments, of 39,238 defects. During the year ending December 31st, 1911, the total number of eye, ear, nose, and throat defects and ringworm cases treated was approximately 65,000, and during the year ending July 31st, 1912, 60,000. It was estimated that one-third of the defects would be dealt with apart from the arrangements made by the Council. On this basis, and taking 60,000 as the number of defects which would be found on inspection to require treatment during the year 1912–13, provision for the treatment of 40,000 was necessary. The subcommittee was of opinion, however, that provision for the treatment of 39,238 defects would be sufficient, so far as that proposed for eye, ear, nose, and throat defects were concerned, but it was possible that some extra accommodation for the treatment of ringworm might be needed. The question of some further provision was receiving consideration.

The authorities of the institutions included in the scheme sanctioned by the Board of Education until December 31st, 1912, were willing to renew the agreements, but in five cases this consent was conditional. The following particulars were given with regard to the more important conditions:

1. *Deptford Children's Health Centre.*—The committee was unwilling to allow the Council to select the surgeons, as was now the case at treatment centres provided by medical practitioners.
2. *Miller General Hospital.*—The governing body was willing to renew the agreement in the event of the Council not being able to make other satisfactory arrangements.
3. *The Metropolitan Ear, Nose, and Throat Hospital.*—The governing body required that the capitation payment should be increased to 6s., as the anaesthethists now required payment.

The subcommittee recommended that the existing agreements with the London, the Miller General, the Royal London Ophthalmic, and the St. Mary's (Paddington) Hospitals be renewed upon the present conditions until July 31st, 1913, and that the agreements with the undermentioned hospitals and centres be renewed until July 31st, 1913, with the modifications indicated in each case.

1. *Belgrave Hospital for Children.*—The number of ear, nose, and throat cases to be dealt with to be reduced from 1,500 to 1,000 a year.
2. *Charing Cross Hospital.*—The number of eye cases to be dealt with to be reduced from 500 to 250 a year.
3. *Deptford Children's Health Centre.*—The selection of the doctors to be made by the Council from nominations made by the committee of the centre; the services of an anaesthetist on two sessions a week to be utilized, and the agreement to provide for the length of the session to be two and a half hours.
4. *Metropolitan Ear, Nose, and Throat Hospital.*—The capitation payment to be increased from 4s. to 6s. a case.
5. *Norwood School Treatment Centre.*—The number of eye (refraction) cases to be increased from 440 to 880 a year, and the number of ear, nose, and throat cases to be reduced from 1,320 to 440 a year. The committee to be guaranteed against loss a sum not exceeding £15 in the event of the agreement terminating on July 31st.
6. *Woolwich and Plumstead Cottage Hospital.*—The agreement to be renewed, subject to satisfactory premises being provided; and the committee of the hospital to be informed that it was not possible to accede to a request for an increased grant in respect of nursing treatment.
7. *Wandsworth School Treatment Centre.*—The number of ear, nose, and throat cases to be dealt with to be reduced from 1,320 to 880 a year, and the number of ringworm cases to be increased from 150 to 220 a year.
8. *Royal Eye Hospital.*—Provision to be made for the treatment of 4,000 cases a year, but unless the committee of the hospital was prepared to make arrangements for the treatment in the special children's department of all the children attending the hospital from London elementary schools, the Council to guarantee payment on 3,000 only.

The subcommittee did not propose to renew the agreement with the governing body of the Hospital for Sick Children, Great Ormond Street, owing to the insufficiency of cases to justify special arrangements for treatment being continued. The committee described in detail the arrangements which are to be continued at the various hospitals and centres for "following up" after-treatment.

Considerable discussion took place at the meeting of the Education Committee on the question of the continuance of the present arrangements, the statement being made that the attempts to secure treatment through the agency of the voluntary hospitals had been, on the whole, unsatisfactory.

The Chairman of the Subcommittee (Mr. Anderton), however, declared that in all cases the arrangements now proposed to be made would follow the lines of those which had received the entire approval of the Board of Education as carried out at the London Hospital.

Major Levita appealed for the inclusion of the treatment of tuberculosis in the Council's scheme.

Mr. R. A. Bray moved an amendment to provide that in districts not comprised in the area of any of the proposed schemes one or more school clinics, under the direct management of the Council, be established, in order that their value might be tested, and that they might afford a guide to the proper sum which should be paid in grants towards the maintenance of centres privately established.

This proposal was rejected by 19 votes to 10.

Dental Treatment.

The subcommittee also stated that it had had under consideration arrangements for the dental inspection and treatment during the year 1912–13 of children attending elementary schools.

In order that dental treatment might be of permanent value, it was necessary that arrangements should be made for each child to be reinspected by the school dentist, and any further treatment which might be required as the result of such re-examination carried out. It was proposed that the area from which children attended the centres should be so arranged as to enable the inspecting dentist to visit each school once a year. Children examined for the first time when 6 or 7 years of age would come under the notice of the inspecting dentist when 7 and 8 years of age respectively under the existing scheme, but it would be necessary to make special provision for the reinspection of children after they reached the age of 9. Arrangements for such an extension of the scheme will shortly receive consideration.

With regard to the accommodation for treatment for the year 1912–13, the Board of Education had already approved arrangements for treatment at the Fulham Dental Centre, the St. Marylebone General Dispensary, and the Wandsworth School Treatment Centre until July 31st, 1913, and it was suggested that the four agreements provided for the dental treatment of children, which would expire on December 31st, 1912, should be renewed. The authorities of the centres were willing to continue the work, subject, in the case of the St. George's Dispensary, to the Council allowing a nurse, as at the Norwood and Wandsworth centres. This had been approved. The committee of the Deptford Children's Health Centre objected to the selection of the dentist by the Council, but the subcommittee did not see its way to waiving this condition. In the cases of the Deptford Children's Health Centre and the St. George's Dispensary, it would not be necessary to guarantee a minimum capitation payment. It was proposed to open additional centres at Islington, Peckham, St. Pancras, and Woolwich. Additional inspecting dentists and three additional temporary school nurses would be required in connexion with the proposed new centres, and the provision of two other centres was under consideration. The cost during the first year of the four new schemes would be £1,976, excluding administrative charges.

A proposal that provision should be made for the dental treatment of not less than 50,000 cases a year was rejected by 16 votes to 14, Mr. Anderton stating that it was difficult to persuade parents to send their children for dental treatment, and that in the present state of public opinion treatment could not be secured for so large a number as 50,000.

Ireland.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

Private Practice of Public Medical Officers.

A new indignity has been thrust upon the medical profession in Ireland. Hitherto a dispensary doctor in the country, though he had to discharge his public duties for a miserable pittance, after submitting to election by men who, as a rule, were entirely incapable of discriminating between the qualifications of candidates, had the right to charge what fees he liked for any private practice he was able to obtain in his district. Now this is to be done away with. The Swinford Board of Guardians at their last meeting decided to adopt the following scale of fees for private practice, to which the candidates must agree before appointment.

Midwifery Cases.

When a call is made between 7 a.m. and 7 p.m., £1; when the call is between 7 p.m. and 7 a.m., £1 5s.; for each subsequent visit, 5s.

All other Cases.

For the first visit, 10s.; for each subsequent visit, 5s.

Attendance at Medical Officer's Residence.

Fee to be charged to any person who attends at the doctor's residence for advice and treatment and unable to pay the doctor's usual fee, 2s. 6d. These fees shall include the supply of all necessary medicines, which must be given by the doctor at his own expense.

The Problem of Hungry School Children.

A public meeting was recently held at the Mansion House, Dublin, in support of the Ladies' School Dinner Committee. Sir Charles Cameron, who occupied the chair in the unavoidable absence of the Lord Mayor, stated that the city was, on the whole, a poor city; that an unduly large proportion of the people of the city were very poor was shown by the fact that though there were 32,000 houses in Dublin, 300,000 people lived in less than 6,000, and that 30 per cent. of these families had each only a single room. In other parts of the United Kingdom the children of the poor enjoyed advantages not possessed by those in Ireland. There was no organized medical inspection as in England. The authorities in England had been given power to supply food for children who were in very indigent circumstances. In Ireland the authorities had no such power. The imposition even of one penny on the rates would produce a sum that would meet the wants of a very large number of children. The following resolutions were passed:

That this meeting calls upon the Irish members of Parliament to get the Free Meals Act extended to Ireland in a form suitable to this country.

That, failing the extension of the Act, the Corporation of Dublin should make a grant to meet the immediate needs of the school children.

At present such a rate would not be legal, and the Corporation would be liable to surcharges for it, but it is hoped that leave will be obtained from the Local Government Board to raise the necessary money.

Royal Veterinary College.

At the opening of the thirteenth session of the Royal Veterinary College of Ireland, Professor J. J. O'Connor, M.R.C.V.I., delivered an inaugural address. He stated that it was estimated that about 30 per cent. of the cattle in Great Britain and Ireland were tuberculous, although the majority of these appeared in perfect health; the veterinary surgeon, however, had in tuberculin an infallible diagnostic agent for the disease in the living animal, even when in a latent form, and from his special training in veterinary pathology he could readily recognize the tuberculous udder and carcass, and was thus in a position, if called upon, to protect the public against the ingestion of milk and flesh containing the germs of the disease. The transmission of the disease from bovines to man could be largely, if not entirely, prevented by employing the services of the veterinary surgeon in the inspection of dairy cows and meat. Private abattoirs would soon be abolished, and every town would have its veterinary meat inspector, so that the inhabitants of Ireland would no longer be at the mercy of the ignorant or unscrupulous meat vendor, who was at liberty to sell to them, without fear or hindrance, the carcasses of all the diseased cattle in the surrounding country. With regard to the recent outbreak of foot and mouth disease in the country after a freedom from it of thirty years, it might not have proved so serious if a veterinary surgeon had been called in to attend the sick animals instead of a quack, for he would have diagnosed the nature of the disease at once. A Departmental Committee of veterinary experts, he said, had just been appointed to investigate the disease, the locale of the inquiry being India, where there was unlimited material for the purpose, the contagion being widespread throughout that country.

Trinity College Bicentenary Memorial Fund.

At a meeting of the medical graduates of Trinity College, held last July during the bicentenary festival week, it was decided to raise a fund by subscriptions from old students to establish a permanent memorial. It is hoped that the fund will provide a second travelling prize. Already a considerable sum has been promised, and a list of subscriptions is to be published at an early date.

Housing and Town Planning Association.

Last week a conference of members of this association was held in Dublin, the Countess of Aberdeen presiding. In addressing the meeting, Lady Aberdeen gave figures from the returns of the last two censuses showing what had already been done in Ireland to improve the dwellings of the poor. The homes throughout the country were divided into four classes—the old cabin of one room with one window, the third class with one to four rooms and windows, the second class with five to nine rooms, and the first class with nine or more rooms. During the last ten years first-class homes had increased all over Ireland by over 11 per cent., and second-class houses by 12 per cent.; third-class houses had decreased by 26 per cent., and fourth-class houses by 50 per cent. The percentages in towns were very similar. There had been a reduction of one-roomed tenements throughout Ireland of over 20,000 during the last ten years. The aim of the association, she said, was to bring together all earnest social workers who felt the burden of the present housing conditions. The following motions were carried:

1. That the urgent problems of housing and economic planning of urban and other areas throughout Ireland demand united and educative action on the part of the urban authorities, voluntary organizations, and others interested.
2. That the State should provide financial facilities for urban and other housing schemes not less favourable than have been provided for those in rural districts under the Labourers' Acts (Ireland).
3. That legislation codifying existing statutes and extending the powers of urban and other authorities for the acquisition and development of sites for housing schemes is urgently necessary.

In the evening Mr. Henry Vivian, J.P., Chairman of Tenants' Co-partnership, Limited, delivered a lecture on housing and health.

Scotland.

[FROM OUR SPECIAL CORRESPONDENTS.]

Medical Conditions in the Highlands.

The committee appointed by the Treasury to inquire into the medical, sick nursing, and hospital provision in the Highlands and Islands held a meeting in Oban on October 28th. The evidence received was entirely from members of the medical profession in Argyllshire. The witnesses were Dr. Roger MacNeill, M.O.H. for the County, Drs. Macnicol, (Taynuilt) Grant (Ballachulish), Mackay (Appin), MacDonald Ross (Morvern), and Mackechnie (Bunessan).

All the witnesses gave great prominence to the desirability of increasing the emoluments of the profession, more especially in outlying parts, insisting that at present inadequate medical attendance was due to insufficient remuneration. A minimum salary, it was urged, should be assured, and with a view to that and for other reasons a State medical service was strongly recommended, local resources having already been strained to the utmost in endeavouring to attract competent medical men. It was also held that the efficiency of medical attendance was hampered by the existing parish delimitations, and that it was very desirable that there should be a redistribution in the medical administration areas. A strong contrast was drawn between the position of the ecclesiastical and medical professions in several of the parishes, one parish in the county being mentioned where there were about a dozen churches and manses and only one doctor's house, and that inferior to any of the manses.

The nursing system in the county, which is mainly controlled by the Argyllshire Nursing Association, was found to be superior to any as yet investigated by the committee, but the number of nurses, according to the evidence, is still inadequate, several of the island parishes being without any nurse. Strong testimony was borne to the high qualifications of the nurses and their devotion to the service. A real drawback in the nursing service was stated to be the difficulty at times of finding suitable

accommodation for the nurse when a case demanded her absence from home; a bell tent had in some cases been provided for her accommodation.

Other suggestions for the improvement of the medical service were the provision of good seaworthy motor boats in certain places and motor cars for doctors, the development of the existing post-office telephone system, with a view to making it available for direct communication not only between doctor and patient, but also between doctor and nurse; the advantages in the case of the islands were held to be obvious.

Dr. Roger MacNeill gave a full account of the efforts made by the local authority to combat tuberculous disease in the country, and to the invaluable service rendered by the Argyllshire sanatorium at Oban. At present one of the principal difficulties was that the majority of cases were received only after they had entered on the incurable stage. Domiciliary treatment of the disease had not yet been carried out to any extent.

The committee afterwards visited the sanatorium, and the members expressd themselves highly satisfied with the institution and its situation.

The committee sat again at Oban on the following day to hear witnesses from South Uist and Barra. Dr. Riordan (Daliburgh), the Rev. Father MacDougall (Daliburgh), the Rev. Father Hugh Cameron (Castlebay), and the Rev. Father Macphail (Eriskay) gave evidence which went to show that in these islands the problem of medical attendance was peculiarly difficult owing to their isolation and the inability of the great bulk of the people to pay adequate fees. Medical men were not easily attracted to the islands, nor did they remain long even when the parish council paid a salary out of proportion to the number of paupers for whom the doctors were ostensibly appointed. The service was detrimentally effected by frequent changes. To retain the services of doctors resort had been had to a club system by payment of 5s. per "smoke," but so far this method had been unsuccessful, and the total amount received from club fees and the salary paid by the parish council was not by any means adequate remuneration. A large amount of illness was stated to be unrelieved by medical treatment. In one district this was put as high as 40 per cent., and this estimate was borne out by the high percentage of uncertified deaths. The difficulty of travel throughout the islands was also dwelt upon, and mention was made that even if a doctor could afford to keep a motor car or motor cycle, the people strongly objected to their use, as, owing to the unfenced roads and the nature of the country, they feared their horses would be scared. The necessity of a nursing service was strongly urged. There was no nurse, it was stated, in South Uist, with a population of 5,000 people, and in Barra there was one, and she had only a maternity training. In the case of the island of Eriskay, owing to the difficulty of access in the winter time, a resident nurse would be very useful. Tuberculous disease, both pulmonary and non-pulmonary, had spread rapidly in recent years, and demanded the immediate attention of the authorities. It was held that its prevalence was due, to a large extent, to underfeeding and bad feeding of infants. Hardly any mothers nursed their children, who were almost from their birth fed on oatmeal; there was at all ages an excessive indulgence in over-brewed tea. A regrettable fact was that the cow's milk, which should be devoted to the infant's use, was given to the calf to force its growth. Eggs were also seldom or never used, but bartered at the local merchant's for tea and other commodities. Physical deterioration consequently resulted, and the capacity to withstand tuberculosis or other infection was decreasing.

From the Arisaig and Knoydart district, the Very Rev. Canon Chisholm, Arisaig; the Rev. Angus Macrae, Morar; and Mr. James Macrae, Knoydart, gave evidence, dwelling especially on the difficulties in the parishes of Glenelg, Arisaig, and Moidart owing to the sparse and scattered population, the mountainous character of the country, which was largely intersected by arms of the sea, making it extremely difficult for doctors and nurses to reach patients.

The committee continued its sittings at Oban on October 30th. Witnesses examined from the remote districts of Ardnamurchan and Acharade were the Rev. John Smith (Kilchoan), Mr. J. Campbell (crofter, Kilmorey), and Dr. Macnaughton (Salen, Loch Sunart), who has been in practice in the parish of Acharade for thirty-two years. The needs of that district have been brought under the notice of the Local Government Board; the witnesses examined at this sitting made a strong case for establishing a resident medical man at Kilchoan, and paid a tribute to the generosity of Mr. Rudd, the proprietor, in aiding in the provision of nurses throughout the district. For the whole district of Lochaber the case was presented by Dr. de Sylva (Arisaig), Dr. Brander (Fort William), Canon Macintosh, Mr. George Malcolm, and Provost Young (Fort William). From their evidence the question of medical attendance presented much the same features as in other similarly situated districts visited by the Commission. Special reference was made to the part played in the prevalence of tuberculosis by the milk supply, and to a discovery recently made as to the spread of the disease through the medium of the crofters' milk cows, with which the sanitary authorities are less able to deal than in the case of the farmers' byres, which are usually registered. The closing of the Invergarry railway was referred to as having affected the medical service in the eastern part of Lochaber district very adversely. The Rev. Malcolm MacCallum, Chairman of the Lorn District Committee of Argyll County Council, gave evidence as to the medical service in the county and in the Highlands generally, particularly from its financial aspect, and made suggestions as to the local and imperial grants for the betterment of the existing inadequate arrangements.

At the meeting of the committee on November 1st, which was held in Glasgow, evidence was taken as to the working of the dispensary system of medical attendance in Ireland, and more particularly in the poorer and sparsely populated districts. The two medical witnesses were Dr. Coey Biggar, medical inspector under the Local Government Board, and Dr. Warnock, a medical practitioner from Donegal. The statements submitted showed that the doctor's total emoluments, notwithstanding the heavy rating and the somewhat liberal Government subvention, were not always attractive, nor was there such a method of promotion as might be expected under a system controlled by a Government department. The security of tenure, subject to an appeal to the Local Government Board, was an advantage. As regards medical administrative areas, the nursing and hospital provision in Ireland appeared to be much superior to what obtained in Scotland. A striking fact, however, was that, notwithstanding the very complete system of medical attendance, the uncertified deaths in certain districts of the West of Ireland were still as high as 40 per cent.

Dr. Gilmour (Islay), Dr. Hunter (Lochgilphead), and Mr. Thomas Kay (Ardfern) were also examined as to the medical provision in their respective districts.

The committee held its final sitting in Glasgow on November 2nd. The witnesses examined were Her Grace the Duchess of Montrose, Cameron of Lochiel, Sir Donald MacAlister, and Mr. J. R. Motion, Inspector of Poor, Glasgow.

The Duchess of Montrose gave evidence as President of the Govan Nursing Institute regarding the system of modified training of nurses intended for service, more particularly in the rural parts of the country, which she had been able to establish in that institution. These nurses were doing excellent work in such parts of the Highlands as were fortunate enough to secure their services, but there were far too few of them. It was strongly held that further means should be devised to extend this beneficent provision by the training of many more women for attendance on the sick poor, and more especially on cases of maternity. She hoped that steps would be taken at an early date to increase the training capacity of the institute.

On the general question of medical provision valuable information and advice was given by Sir Donald MacAlister, Principal of Glasgow University. From his intimate knowledge of the Highlands and of the medical provision therein, and more particularly in his capacity as President of the General Medical Council, his opinions as to the solution of the problem before the Commission were regarded as of the greatest importance. The character of the service generally as regards the personality of the medical man, his environment, remuneration and prospects, and the too often precarious nature of his

tenure of office, were discussed, as were also the most feasible means for the amelioration of existing conditions as regards not only better remuneration, but also the more direct control of appointments and dismissals, with special reference to a gradual development of a system which would lead eventually to a State medical service. As a special factor in the problem of adequate medical service, prominence was given to the fact that the majority of medical practitioners, owing to the expense more particularly of providing a locumtenent, rarely got a holiday. This was regarded as detrimental, not only to the doctor, but to the patients. A holiday would not only recoup the doctor and enable him to stand the strain of country practice, but if, as was frequently the case, he attended a post-graduate course at a university centre, his efficiency would be very materially advanced in another direction. Most doctors, again, made but a bare subsistence, and consequently could not provide for old age, the result being that many of them had to continue in practice after age and infirmity had impaired their efficiency for such practice as was expected of them. In these circumstances the advisability of recommending a scheme of superannuation was pressed upon the Commission.

Interesting information was also supplied by Cameron of Lochiel as to the general conditions in Lochaber, and more particularly on his own extensive estates, where a club system of medical attendance, which included whole families, had been in vogue for many years. The effect thereon of the Insurance Act and its adaptation to the special circumstances were discussed.

Mr. J. R. Motion, Inspector of Poor, gave evidence as to the arrangements for medical attendance on the 2,146 boarded-out children under the jurisdiction of the Glasgow Parish Council. The attendance was satisfactory, and he bore testimony to the spontaneity with which the medical men had aided the council in the discharge of this most important part of their public duties. A further and most important step had recently been taken in the provision of expert dental treatment for those children, who were now, in that and other respects, much better off than the native children of the Highlands and Islands.

The committee has spent six weeks in visiting the Highland counties, and has examined over 200 witnesses. It is expected that its report and recommendations will be issued early next year.

Incidence of Ophthalmia Neonatorum in Scotland.

The Local Government Board for Scotland has issued to local authorities in Scotland a circular and an accompanying report by Dr. T. F. Dewar, one of their medical inspectors, on the incidence of ophthalmia neonatorum in Scotland. It is pointed out that local authorities can apply to it the provisions of the Infectious Diseases (Notification) Act. Numerous local authorities in England have made ophthalmia neonatorum notifiable, but only a few Scottish authorities have done so. Statutory notification prevails throughout Switzerland, Germany, Austria, France, and in fifteen of the United States of America, the aggregate population of the last being upwards of 38 millions.

A report on this subject was issued by the British Medical Association in 1909. Its conclusions were to the effect that:

1. Ophthalmia neonatorum accounts for upwards of 10 per cent. of all cases of blindness.
2. The number of cases of ophthalmia neonatorum shows a slight but steady decrease, so far as can be judged by returns from British lying-in hospitals and departments and from eye hospitals.
3. It is still, as it has been for many years, the cause of at least one-third of the blindness in all inmates below the age of 16 or 18 years in British blind schools and asylums; and
4. Ophthalmia had been found to occur amongst cases attended by medical practitioners, as well as amongst those attended by midwives.

The last conclusion is significant in view of the statement that, if suitable precautions are taken, the disease can be prevented.

Dr. Dewar conducted an inquiry in twenty of the largest towns in Scotland, directing his attention in particular to the following points in view:

1. The present incidence of ophthalmia neonatorum expressed as far as possible statistically, and its relative incidence in past and present periods.
2. The extent to which it is at the present time a cause of blindness, both absolutely and relatively to other maladies productive of blindness in youth.
3. What proportion of cases of ophthalmia neonatorum are due to gonococcal infection and therefore directly associated with venereal disease.

The conclusions arrived at as a result of these inquiries are:

1. That it is the general but not invariable belief among those who are engaged in ophthalmic work and general practice in Scotland that ophthalmia neonatorum is less prevalent than it was twenty-five or even ten years ago; that actual loss of vision from this cause is less frequent than heretofore all are agreed. Nevertheless, the disease is not so uncommon, nor grave impairment of vision as an effect of it so rare, that it can be regarded lightly or with indifference. Basing an approximate estimate on a review of all the facts and opinions which he had been able to glean, Dr. Dewar says that it would seem that even at the present time about 500 cases occur annually in Scotland, and that of these 30 to 40 suffer some impairment of vision, and 5 to 10 become economically blind. Dr. Dewar states that these figures understate rather than exaggerate the truth.
2. Among those with whom Dr. Dewar discussed the question of ophthalmia neonatorum and its prevention, a considerable majority expressed an opinion in favour of its inclusion among the notifiable diseases; he also expresses the opinion that if the facts regarding the prevalence, preventability, and dire results of ophthalmia neonatorum were brought prominently to the notice of local authorities, further action, based upon the circumstances and needs of each area, might well be left to the initiative of the local authorities themselves.
3. The opinion was submitted very generally that, apart from or in addition to statutory notification of ophthalmia neonatorum, much might be done to restrict its incidence and still more to control its ravages were a Midwives Act for Scotland (on lines generally resembling the English Act) to be passed by Parliament.

School for Defective Children at Paisley.

The latest addition to Paisley's educational equipment is a new school for the training of defective children, formally opened on September 24th, and constructed to accommodate 320 pupils. Outside Glasgow, Edinburgh, and Govan (to be incorporated with Glasgow next month), this is the first school of its kind in the country, and is fitted with all modern requirements for the training of children suffering from physical or mental disabilities. For the physically defective there are on the ground floor eight class-rooms, each accommodating 20 pupils. Play hall and dining hall are conveniently situated, the latter accommodating 160 children. The mentally defective have eight similar class-rooms on the first floor. There are four playgrounds, each of which has a sand-pit for recreation purposes, and the front portion of the site is laid out in plots for special instruction in the cultivation of shrubs and plants. An open-air class-room, with a system of folding doors for protection from wind and rain as required, has been provided for tuberculous children. The cost of the building is £9,600, and pupils will be admitted from Renfrew, Barrhead, Neilston, and Johnstone districts as well as from Paisley. Physically defective children will be brought to school in special motor conveyances.

At the municipal elections in Portsmouth, contests took place in ten out of fourteen wards. In two there were medical candidates, and in one Mr. C. P. Childs was returned by a majority of 57 votes.

The "Pass the Bill" Committee proposes to hold a demonstration in the London Opera House, Kingsway, on the evening of November 12th, in support of the Criminal Law Amendment (Commonly called the White Slave Traffic) Bill. Many important societies—religious, social, and political—are uniting to give further publicity to the strong determination daily growing in force that this bill shall be passed without the weakening amendments made in Grand Committee. Particulars of the meeting can be obtained from the Secretary, "Pass the Bill" Committee, 19, Tothill Street, Westminster.

The Lord Mayor, attending in semi-state, opened on October 31st a considerable extension of the business premises of Messrs. John Bell and Croyden. In the course of a short speech, Sir Thomas Boor Crosby mentioned his friendship with a son of the John Bell who founded the business in 1798. The original shop was a well-known landmark in Oxford Street until some four years ago, when it was amalgamated with the business founded by John Bell's original assistant, and moved to 50, Wigmore Street, under the title of John Bell and Croyden. A feature of the firm's new departure is an arrangement for the hire of all appliances required in the sick-room.

Correspondence.

ACCOUNTS FOR FEES UNDER THE NOTIFICATION ACT.

SIR,—A matter has been occupying some of my attention officially lately which I think is worthy of recording in the JOURNAL, for the benefit of the profession in this country. Under the Notification of Diseases Act, as is known, certificates are required to be sent in to the medical officer of health by practitioners attending persons who are the subject of such diseases as are notifiable.

For these certificates a fee is payable by the local authority. A procedure has lately been adopted by many of these authorities under which a form is issued to practitioners with a demand that this should be filled up giving a detailed account of these certificates, and a statement is made that unless this account be made up no fees will be paid under the Act; in one instance brought to my notice an actual refusal was made and the fees withheld. It cannot be too widely known that this demand for an additional "account" is without any legal authority at all; and that if a practitioner is refused his fees on the ground that he has not made up this account such refusal is absolutely illegal.

In order to make this clear I brought the matter before the Local Government Board, and received a reply which will, I trust, prevent this impcsition on the part of local authorities from being repeated. From the reply I herewith extract that referable to the point involved:

> I am to state that they have informed the Council that the Infectious Diseases Act, 1889, requires the local authority to pay for each certificate, and that the Board are not aware of any legal obligation resting upon medical practitioners to send to the authority a statement of fees apart from the certificates themselves.

This letter is signed on behalf of the Local Government Board by the Assistant Secretary, and is dated September 24th, 1912. I had a similar reply and statement in December, 1911, from the Local Government Board in respect of the action of local authorities requiring medical practitioners under the Tuberculosis Regulations, Article VI, to submit "accounts of fees" due in respect of such notifications. I trust, therefore, that this extra task which the local authorities have endeavoured for some time illegally to cast upon medical men will not be countenanced or acceded to by them. Our duties of notification rest with the issue of the certificates, and the payment, small as it is, automatically follows the issue, but as a matter of convenience we have permitted it to remain in abeyance until the quarter following the date of the notification.—I am, etc.,

A. G. BATEMAN.

Medical Defence Union,
4, Trafalgar Square, W.C.,
Nov. 4th.

GENERAL PRACTITIONERS AND CONSULTANTS.

SIR,—Will you grant me a little space to draw attention to a matter which seems to me to open up the question of the relationship of the general practitioner to the consultant?

The Council of the British Medical Association has made an appeal for no mean subscription to its Defence Fund, as well to the consultant as to the general practitioner. Now, it is obvious that the Insurance Act, which is admittedly seriously endangering the position of the latter, is of far less serious importance to the former. If, then, the practitioners of the profession expect the consultants to help them in their fight, is it not only just and fair that they should begin to regard them in a less prejudiced light than has too often been the case hitherto, and look upon them rather as willing colleagues and helpers than as being a body of men who are always ready to "damn by faint praise" and rob them of their patients when sent to them? That there are black sheep in every fold must be admitted; but such are pretty generally recognized, and can therefore be avoided; but that the main body of the consulting branch of the profession should be tarred with the same brush is neither fair to it nor creditable to its sister branch. Repeatedly one hears accusations made against consultants for "seeing patients without the knowledge of the doctor," for "retaining and treating patients sent to them," and for "operating upon patients without informing the doctor." When will the general practitioner recognize the fact that, no matter what rules we as a profession may make, the public is, and will insist on remaining, a perfectly free agent, considering itself at liberty to go to whatever doctor it pleases and whensoever it pleases? When will he understand that at times a patient will tie the hands of the consultant by absolutely refusing to allow him to communicate with his doctor, and will, as has happened to me on several occasions, transfer his case to another consultant if he refuses to examine him in the absence of such permission? Again, does the general practitioner always "play the game" as he expects the consultant to play it? Let me give an instance: Comparatively recently three patients, all in need of operation, came to me without the knowledge of their doctors, not knowing the rule. All were willing that I should communicate with these gentlemen, and in each case I did so. What was the result? In not one of the cases did I receive the courtesy of a reply, nor did any of the patients return to me for operation.

I have in my possession a letter from a general practitioner whose patient, coming to me without his knowledge, absolutely refused to allow me to write to him till her operation had been done, threatening, if I refused to see her, to go elsewhere. The operation was for a scirrhus of the breast, which both her doctor and his partner assured the lady did not exist. (I sent these gentlemen the microscopical proof of this cancer later.) The letter accuses me of diagnosing a cancer which was not there; of "sharp practice" in taking their patient; and of contravening the ethics of the profession.

I have no wish, Sir, to raise an acrimonious discussion, nor to induce a "washing of dirty linen" in the press, but I do think that the time has come when some steps should be taken to clearly define the relationship of the consultant and general practitioner, and to recognize the indisputable right of the public to be considered free agents and not the prescriptive right of the first medical man they happen to go to. For, until this is done, and the honour and honesty of the consultants cease to be fair game for those who so frequently impugn them, it is hardly in the best of taste to expect the consulting branch of the profession to put its hand deeply into its pocket to help those who are so ready to throw stones at it.

For the sake of the many loyal and good friends I have among the general practitioners, I am willing to help to the best of my ability, but I am sure that many of my fellow-consultants will support me in my contention, and hope that something may soon be done towards creating a better understanding between the two branches of our profession.—I am, etc.,

LAWRIE MCGAVIN.

London, W., Nov. 1st.

THE SURGICAL TREATMENT OF RECTAL CANCER.

SIR,—It was my privilege to hear the able address delivered by Mr. Harrison Cripps in Liverpool in July as an introduction to the discussion upon rectal carcinoma, when I was struck by, and not a little surprised at, his remarks upon the value of the sigmoidoscope in diagnosis. I was diffident about criticizing the views of so distinguished an authority at the time, but as the question has been raised by your correspondents, Mr. Lockhart Mummery and Mr. Gordon Watson, my old chiefs at St. Mark's Hospital, I may venture to add a brief contribution to the discussion and to join them in advocating the claims of the sigmoidoscope.

On referring to the notes of the cases in which I have employed endoscopy during the present year, I find no fewer than five examples of carcinoma which defied diagnosis by digital examination as the growth was beyond the reach of the finger, and in which an accurate diagnosis was arrived at by the aid of the sigmoidoscope. In three of these, in order to put the question beyond doubt, small portions of the growth were removed by long forceps and submitted to microscopical examination, with the result that the pathological reports confirmed the clinical diagnosis.

In another case—that of a man in whom a high rectal carcinoma was suspected on account of the fact that the

finger reached the lower edge of a mass involving the bowel wall—a small portion of the overlying mucous membrane was removed through the sigmoidoscope and shown on microscopical examination to be normal, giving rise to the suggestion that the swelling was not a primary carcinoma of the rectum, as was proved by the after-events.—I am, etc.,

HAMILTON DRUMMOND, F.R.C.S.Edin.

Newcastle-upon-Tyne, Nov. 4th.

SIR,—I have read with much interest both the kindly remarks and criticisms by Mr. Lockhart Mummery and Mr. Gordon Watson on my address at the Liverpool Annual Meeting on the surgical treatment of rectal cancer (BRITISH MEDICAL JOURNAL, October 5th). As to the criticisms, I, like St. Paul, am willing to justify myself.

At the meeting a printed table (Table A) was handed round. Owing to its length (seventy-two pages), this was not reprinted in the JOURNAL, but is accessible. The table gave the details in chronological order of every case of rectal cancer seen by me in private practice. It was explained that hospital cases were omitted owing to the difficulty of following the subsequent history.

The table in each case gives the date, the name of the medical attendant, sex, age, duration of symptoms, *height of disease* from anus, treatment adopted, and result. Owing to the date and name of medical attendant, an opportunity is offered of correcting any error.

Part of the criticism of your correspondents was directed to my statement that "in an overwhelming percentage the disease is situated within six inches of the anus, probably not 10 per cent. originating above that site." In the table referred to the position or height in inches is given in 442 cases out of 445. In 381 cases the disease was within six inches, or 86.4 per cent. The remaining 61 cases are described as "high up"—that is, above measurable distance but not necessarily above six inches. This amounts, if all were included, to 13.7 per cent.

My rectal cancer cases have always been measured with the same keenness as a big game shooter measures his heads, and I am confident of their general accuracy, and they support my statement that, "as shown by Table A, an overwhelming percentage of cases are situated within 6 in. of the anus." Assuming that all the 61 cases described as "high up" were above 6 in., the figure would be 13.8 per cent.; but as some of these cases were undoubtedly, though not measurable, within 6 in. of the anus, the statement that "probably only about 10 per cent. are above this height" is substantially correct.

In considering the wide divergence between my statistics and those given by your correspondents, it must be remembered that all my cases were in private practice. The great majority of these were sent to me by their own medical attendant, who had already detected the disease by digital examination. While it is not unlikely that some of the higher cases, that is, beyond digital detection, had been referred to the physician as "colitis," or some trouble high up.

Mr. Mummery gives his measurements "as above and below the peritoneal reflection," apparently considering this as the equivalent to my "6 in. above the anus," but it should be remembered that experimental injections of plaster-of-Paris show that 3½ to 4 in. is the average height of the reflection, and not 6 in.

Your correspondents infer that had I made more use of the sigmoidoscope more higher cases would have been detected. This is not so. If symptoms of cancer are present and an ordinary examination has failed to detect a growth, a thorough bimanual examination is resorted to under an anaesthetic, and if disease is present it is very rarely missed. It is in these cases that the sigmoidoscope has failed to assist me.

Mr. Mummery says cancer "can be much more certainly diagnosed by sight than by touch." This is not so—at least, to me, and I adhere to my statement "that cancer can never be diagnosed to a certainty by sight alone; touch only, by indicating hardness and friability, has to be relied on."

This is especially so in an early stage of rectal cancer, where the disease frequently begins as a small flat infiltration in the submucous tissue. At first this is covered by an almost normal-looking mucous membrane, ulceration being a later stage. The fact, however, that it is hard and firmly adherent to the subjacent tissue discloses its malignant nature, and for this touch alone has to be relied on.—I am, etc.,

London, W., Nov. 4th. HARRISON CRIPPS.

RASHES OCCURRING DURING THE PUERPERIUM.

SIR,—May I say, in reply to Dr. Herman's letter in the JOURNAL of November 2nd, p. 1249, (1) that since 1883-84, when Dr. Boxall's sixteen cases of scarlatina occurring during pregnancy and in the puerperal state, which were of a mild type, were observed, bacteriology has become a science, and that it has been shown that secondary septic changes which are apt to occur in the throat, etc., in scarlatina depend upon the presence of organisms (streptococci, etc.) identical with those met with in the uteri of patients suffering, after delivery, from certain forms of puerperal infection (puerperal septicaemia or puerperal fever), and found in fatal cases in the tissues of their bodies? A similar type of secondary septic infection is seen in diphtheria, the only difference being that in diphtheria the specific organism causing that disease has been isolated, while there is still some doubt as to the exact *materies morbi* of scarlatina. (2) Dr. Boxall's results prove the truth of my conclusions that true scarlatina is rarely conveyed to a lying-in woman, and that any risk of its being carried from the septic focus in the throat, etc., may be avoided by modern aseptic and antiseptic precautions which Dr. Boxall rigidly employed at the General Lying-in Hospital. (3) I have been greatly influenced in my judgement on this question of the relationship of scarlatina to the puerperium by Dr. Boxall's monograph, to which I have referred in my paper. It is interesting to find that modern bacteriological research is in accordance with and explains and confirms his careful observations and his practical results. For clearness, let me repeat the conclusions at the end of my paper:

1. Scarlatina is rarely encountered during the puerperium.
2. When it does occur during the puerperal period it is, as a rule, a mild disease, that is, with the use of modern aseptic and antiseptic precautions.
3. What has been called "puerperal scarlatina" is almost always a form of septic infection with a rash resembling the eruption seen in true scarlatina.
4. From a person ill of scarlatina, there may be conveyed to a lying-in woman either:
   (*a*) A true scarlatina; or
   (*b*) Septicaemia, arising from the secondary septic changes which are apt to take place in the throat, etc., in scarlatina.

The former (*a*) occurrence seems to be rare, while the latter (*b*) should be avoided by modern aseptic precautions.—I am, etc.,

Belfast, Nov. 4th. JOHN BYERS.

THE "EDINBURGH REVIEW" ON SECRET REMEDIES.

SIR,—In your article in the JOURNAL of October 26th commenting upon my essay in the current number of the *Edinburgh Review*, I venture to think that you have inadvertently misrepresented the gist of my argument, for your readers must have inferred that I was attempting to defend secret remedies, whereas my chief object in writing the article was to assist in the public exposure of their worthlessness. I believed, however, that before taking legislative measures towards suppressing a trade dealing in millions per annum, and bringing in a revenue of hundreds of thousands to the Treasury, it would not be irrelevant to hear what the trade had to say in its own defence, more especially since an official pamphlet had been issued on their behalf by the section concerned of the London Chamber of Commerce. I was careful to dissociate myself from their arguments; and I confess it never occurred to me that, in merely reciting the defence they had set up, I should be accused of "parading my impartiality" or "opening the case for the proprietors," particularly after I had devoted considerable space to quoting from *Secret Remedies* the analyses of several of the most egregious nostrums I could find, and had

concluded by advising the public "to have nothing to do with secret remedies," but to purchase the two "amusing and admirable volumes," *Secret Remedies* and *More Secret Remedies*. You describe me as an "uncertain guide" because I quoted from the pamphlet the claim that proprietary medicines are unsurpassed for simplicity, uniformity, and cheapness. But really, Sir, I am not responsible for the statements in the pamphlet; and when you rejoin that "it is abundantly proved that secret remedies are not uniform," you are stating a truth of which I furnished ample illustration in the course of my article. Your critic, moreover, is in error in attributing to me an intention of bracketing antipyrin, urotropin, "tabloid" preparations, etc., with such things as tuberculozyne or Beecham's pills. I mentioned those important bodies purely to emphasize the fact that the publication of their composition or method of manufacture had not prevented them from remaining valuable paying properties; and I adduced them in opposition to the plea of the trade that publication of formula must ruin the property. In short, I believe that I am fully as antagonistic to secret remedies as you are, and that I have condemned them in language not less strong than your own. Finally, as to my "parade of impartiality," permit me, Sir, to draw your attention to the advice which you were good enough to give me in your review (June 8th) of my recent book on Bergson's philosophy: "Purely destructive criticism, by the very fact of its obvious lack of sympathy and of the insight inseparable therefrom, is apt to miss its mark."—I am, etc.,

London, W., Nov. 1st. HUGH S. ELLIOT.

ANAESTHETICS IN EXPERIMENTS ON ANIMALS.

SIR,—I find that a passage in the report of the Royal Commission on Vivisection has conveyed the impression that I regard the frequent use of anaesthetics as unnecessary in experiments on animals. I wish to state in the most unqualified manner that I regard the use of anaesthetics as right and necessary in every case where appreciable pain would otherwise be inflicted, except in the cases (provided for in existing legislation) where the object of the experiment is of sufficient importance, and would not be attained if anaesthetics were used, or where, if the animal were allowed to recover, the after-effects of the anaesthetic would cause more pain or discomfort than the operation.

I also wish to state that in any experiments I may do I comply strictly with the existing law, although, as I stated in my evidence before the Commission, it in some respects defeats its own objects and causes much unnecessary trouble to those engaged in the advancement of medical knowledge.—I am, etc.,

London, Nov. 4th. M. S. PEMBREY.

THE SECRET OF LONG LIFE.

SIR,—May I correct the following errors in my letter printed in the JOURNAL of November 2nd, p. 1248:

1. The superscription ought to be "The Secret of Long Life," the word "long" having been forgotten.
2. In the paragraph on "breathing exercises," "vigour" ought to stand instead of "vigors" of the organs.
3. In the quotation from Shakespeare, "a *lusty* winter" ought to stand instead of "a *misty* winter."—I am, etc.,

London, Nov. 4th. HERMANN WEBER.

The Services.

ROYAL ARMY MEDICAL COLLEGE.

WE published last week the address delivered by Sir Henry Morris after he had presented the prizes at the Royal Army Medical College on October 29th. Before the presentation Colonel B. M. Skinner, M.V.O., Commandant and Director of Studies, addressing the Director-General and Sir Henry Morris, spoke as follows:

Ladies and Gentlemen,

I have the honour to report on this occasion, which closes the twenty-first session in London of the Junior Class of Instruction, that the number of lieutenants on probation has been—for the Royal Army Medical Corps 26, and for the Indian Medical Service 12. These gentlemen, after having become duly qualified in civil medical schools and in many cases having held resident appointments in hospitals of this country, have been under instruction at this College in subjects outside the scope of their previous training. These subjects have a special bearing upon hygiene and sanitation, and upon the pathological and traumatic conditions incidental to the soldier, whose service leads him sooner or later into tropical countries. And to these subjects must be added instruction in the administration of the army, especially in relation to military hospitals both in peace and war. Although the curriculum has opened up ground which was entirely new to most, I am pleased to report that the young officers have taken to their work with zeal and energy. The result of their work as shown by the examinations just concluded has been very good on the whole. Some of the candidates, as was to be expected, have exhibited greater powers of assimilation than others, with the result that the relative positions gained at the entrance examinations have become somewhat altered. And these relative positions may undergo further modification at the completion of the course at Aldershot, to which they proceed to-morrow. There are certain prizes awarded to those who excel in the examinations. In announcing the names of the prize winners, I wish to mention those who gained the distinction of "proxime accessit."

Herbert Prize: Highest aggregate	Lt. J. D. Wilson, I.M.S.	530.5/700
First Montefiore (Military Surgery)	Lt. J. D. Wilson, I.M.S.	85/100
Second Montefiore (Military Surgery)	Lt. L. A. P. Anderson, I.M.S.	80/100
Prox. acc.	Lt. T. E. Osmond, R.A.M.C.	77/100
Tulloch Memorial (Pathology)	Lt. R. A. Flood, R.A.M.C.	147/200
Prox. acc	Lt. T. Poole, R.A.M.C.	134/200
Fayrer Memorial (Pathology)...	Lt. J. D. Wilson, I.M.S.	148/200
Prox. acc.	Lt. J. B. Hance, I.M.S.	147/200
Parkes Memorial (First Hygiene)	Lt. J. B. Hance, I.M.S.	145/200
De Chaumont (Second Hygiene)	Lt. E. B. Allnutt, R.A.M.C.	144/200
Prox. acc.	Lt. H. C. Todd, R.A.M.C.	142/200
Ranald Martin (Tropical Medicine)	Lt. J. Hare, R.A.M.C.	95/100
Prox. acc.	Lt. J. B. Hance, I.M.S.	93/100
Marshall Webb (Military Medical Administration) ...	Lt. J. Hare, R.A.M.C.	87/100
Prox. acc....	Lt. L. T. Poole, R.A.M.C.	80/100

I cannot close my report without expressing regret at the loss the College is sustaining by the departure shortly of Colonel Melville, our Professor of Hygiene. His work has been that of a master in his subject, and his labours in the field of the soldier's physical work and training will have lasting results, some of which he will observe in that magnificent army we furnish to India, where he is now going. On behalf of my staff I wish him God-speed.

Surgeon-General Babtie, C.B., Deputy Director-General, after Sir Henry Morris's address made a short speech, appreciative of it, and asked for a vote of thanks, which was accorded with acclamation. The company, numbering about 200, then proceeded to the mess for tea, the Commandant and officers being "at home."

ROYAL ARMY MEDICAL CORPS.

THE King has been pleased to grant his licence and authority to Major Howard Ensor, D.S.O., M.D., R.A.M.C., to accept and wear the decoration of the Third Class of the Imperial Ottoman Order of the Medjidieh presented by the Khedive of Egypt in recognition of valuable services rendered.

THE estate of the late Mr. Clinton Thomas Dent has been declared at a net value of £116,263. His will directs that his medical and surgical books shall be given to the students' library at St. George's Hospital, and that a sum of £1,500 shall be offered to the Belgrave Hospital for Children subject to certain conditions, and that in default of their acceptance it shall be paid to St. George's Hospital.

Medico-Legal.

STEVENS *v.* BRITISH MEDICAL ASSOCIATION.

Continuing his evidence on Wednesday, October 30th, Dr. Dyke Acland said, in answer to Mr. Gregory, that, with regard to the cases referred to by Dr. Evans, he had seen many similar cases cured without Mr. Stevens's remedy. In sanatoriums everything was done which gave the slightest reasonable ground for success. They had sometimes to refuse cases which were hopeless. A considerable number were sent away with the disease arrested, and when he compared the figures with those put forward by one of the doctors called on behalf of the plaintiff he did not think that the plaintiff's figures proved anything remarkable. With reference to the lady who went to South Africa and was cured after using Mr. Stevens's remedy for three months, he thought she was under conditions in which patients do recover entirely. He had heard of numerous "cures" for consumption since he had been in practice, and it was generally alleged that people had been cured by them.

Mr. Tindal Atkinson: How is this evidence?

Mr. Justice Pickford: Mr. Gregory wants to show that the fact that they recovered does not show that the medicine did it.

Continuing, witness said that many people recovered under treatments to which no fair-minded man would ascribe any specific action. The statement made by Mr. Stevens that under his treatment tubercle cavities would fill up so that they could not be discovered showed that he knew nothing about it. It was impossible to replace lung tissue. A statement had been made by one doctor that he had seen tuberculosis of the throat cured by this medicine, and that he had not known such a cure before. If he had never seen a case of tuberculosis of the larynx cured before, that showed that he had not seen much of tubercle. The statement that the lung of a man who suffered from silicosis became as hard as stone was absurd.

The witness was then cross-examined.

Mr. Tindal Atkinson: You treat Stevens's cure with contempt?—Who said so?

I say this from the answers you have given. Do you quarrel with it?—I do.

What is the view you take of it, then?—I don't think it is of any use.

Your view is that it is absolutely useless as a treatment for consumption?—I believe it is useless.

Are you positive that it is useless?—I am positive that is my opinion.

No, no, that won't do. I want a definite answer.

Mr. Justice Pickford: He cannot say more than that; he thinks it is useless.

Mr. Tindal Atkinson: The defence has gone further than this. They do not say it is a matter of opinion.

Mr. Justice Pickford: I do not see that the witness can say more than he has said.

Mr. Atkinson: Why not? I am entitled to an answer to my question.

Continuing, witness said it was his opinion that the medicine was of no use. On the evidence they had heard in the case he thought an honest man could not believe in its efficacy.

Mr. Atkinson: Do you include in that sweeping condemnation all of the doctors called in this case by the plaintiff?—I do not know what they think.

You have heard, sir, what they think. Do you class them as dishonest men?—If the doctors who have heard the evidence still believe in it I shall be sorry to say that they are not honest.

They are dishonest, sir, out with it?—Well, yes, if you like to put it that way.

That is a very strong thing to say?—Certainly.

Continuing, witness said he had not heard the letters which Mr. Stevens had received from doctors in different parts of the kingdom and in South Africa.

Dr. Acland: May I say something, my lord. I ought not to have said that they were dishonest. A man may be mistaken without being dishonest. I am quite prepared to believe that they may honestly have come to an erroneous conclusion.

Mr. Atkinson: You may say that, having given a contrary opinion a minute ago?—You were taking it in a different way.

Counsel then read the anonymous letters from doctors saying that they had applied his treatment with advantage.

"I have read those letters to you," he said, do you say that the person who received them could not honestly believe in the efficiency of the cure?—He had some reason for it, certainly.

In re-examination, witness said that the plaintiff had brought no evidence to prove that he had got a cure for the great white plague, or that his physic destroyed the germ of consumption, nor had he any ground for saying, "When the disease is devouring everything except your soul and bones, send a post-card to Mr. Stevens and he will cure you." Nor was there ground for saying that after five years' sanatorium treatment less than 3 per cent. of the people treated were able to follow their occupation.

At the conclusion of his evidence, Dr. Acland said that he wanted to make himself perfectly clear. He did not attribute dishonesty to those who disagreed with him. No one who knew him would think he did. A man might be honestly mistaken.

Sir R. Douglas Powell, examined by Mr. Gregory, said he had had a long experience of tuberculosis. Having read the short-hand notes of the evidence, he did not see that any cure was to be attributed to the specific action of the drug. He had known recoveries in circumstances similar to the circumstances of each case said to have been cured, and he ascribed those recoveries to appropriate treatment and the natural forces of the patient. Having described the proper treatment of tuberculosis, he said the patient had to fight the disease and the doctor had to help him to do it. He had himself examined a woman said to have been cured by Mr. Stevens. He found no evidence of extensive lung trouble in the past. His experience of patients relating what they had been told—especially ladies—was that they were rarely accurate. He agreed entirely with what Dr. Acland had said as to the specific cases of "cure" mentioned in the evidence.

Cross-examined, he said a man might be perfectly honest and yet be mistaken.

Mr. Atkinson: Would you feel justified in calling the doctors dishonest who say they still believe in the efficacy of Stevens's cure?—I should not call them dishonest, and I would add that my friend Dr. Acland did not do so.

Would a doctor attending a patient be in a better position to judge of the condition of that patient than a person merely reading the shorthand notes of the evidence?—That depends very much on the doctor. I don't think he would necessarily. He might be able to judge better than one who sees what he writes down.

Continuing, he said that he had not tried the remedy himself. Medical men were open to conviction whether discoveries came from laymen or not provided the truth of them was established. Mr. Atkinson read from an article in the British Medical Journal which pointed out that Harvey's discovery, the open-air treatment of tuberculosis, Pasteur's discoveries, Lister's antiseptic system, the laryngoscope, and other things had at first been refused acceptance by a large part of the profession.

Mr. Atkinson: Was not electricity looked upon with suspicion?—It was.

And massage as an unclean thing?—Yes; but your remarks, wherever they come from, are tremendously exaggerated with regard to every point you have raised.

You say you don't agree with anything I have read?—I do not say that.

Continuing, witness said that it would make no difference to his view of a cure that it was introduced by a layman; not, at least, if it was shown to be a cure. In the present case it had been claimed that the cure killed the bacillus and cured the patient, but the drug appeared to him to be worthless.

Dr. Frederick William Price, in the course of his evidence, said that he had seen Mr. Smith, who had been examined by Mr. Stevens. There was nothing abnormal in his lungs. In cross-examination he said that the average course of consumption lasted from five to seven years. It did not follow a flat course, it had ups and downs; and it was quite possible there might be an improvement for some months. It was not right to attribute the improvement to bread pills or to whatever was being administered.

Mr. Atkinson: Would you not consider that a fresh drug should be tried before being denounced?—Not necessarily.

Would not your answer be against the introduction of any fresh drug?—A fresh drug is usually introduced by experiments on animals.

Dr. Arthur Latham, of Portland Place, a member of the Commission on Tuberculosis at Berlin, said that he had studied the treatment of tuberculosis for seventeen years. Twenty per cent. of sufferers from consumption, he told the court, got well, in spite of treatment, or with any treatment they might have. Dr. Latham added that he had seen people get well whom he thought had got no chance. For instance, there was a case of a man suffering from tuberculosis of the throat and lungs. He had advised this man to go into a home for the dying. The man had refused. Instead he went to a farmhouse, and in a year returned recovered. The witness then said, I will go so far as to state that I never venture to say whether a man is going to get better or not.

Counsel: Has change of climate a beneficial effect?—Dr. Latham: It is difficult to say. I remember a man who went to Davos for a year, and then to Colorado, without good results. He then established himself at Southend-on-Sea, and got well.

The witness went on to say that some one—he believed Mr. Stevens—came to him some years back with a material called Sacco. "I told him that Brompton Hospital was always willing to test cures provided all the cards were put on the table, and experiments conducted on animals," said the doctor.

In cross-examination he said he had never tested Stevens's cure.

Mr. Atkinson: Before you condemn a thing should it not be tested medically?—Provided the test is carried out on proper lines.

According to you there is no known cure for consumption?—No known drug will cure it.

And there never will be?—Probably there never will be.

The witness said a new cure for consumption was brought to him every two or three months. "They always say that cures have been effected by it," he said. "They may make the patient feel better for a time, but that is due to the mental effect and hope."

This concluded the evidence in the case.

In the course of his address to the jury, Mr. Holman Gregory said that the interests of the public lay at the very bottom of

the case. It was of the utmost importance that when a man said he had a cure there should be full, free, and fair criticism of his cure. It would be a sad day for England when a jury took a narrow view of a criticism of what such a person stated, and enabled such a man to go forth and say: "All that I have said is right; a jury has said so." It was a remarkable fact that none of the doctors called on the part of the plaintiff continued to use the medicine after it had stopped coming free. There was no evidence to show that it killed the bacilli of consumption. The plaintiff's advertisements said that his medicine cured people suffering from "consumption in the third degree." Fancy the effect of that upon the unfortunate sufferer! The man with one foot in the grave was prepared to fly to any remedy. As to the price of the medicine, no evidence had been given as to the actual cost. The plaintiff's statements were made to catch the eye, and to bring in money. There never yet was a quack advertisement which did not quote cures, and point to cures as the result of the remedy; but they must not overlook the fact that Nature was always trying to cure the patient. He had said: "I have found the cure for the Great White Plague." It was remarkable, although he had given evidence for the plaintiff, Dr. Grün had not continued to use the remedy. Healthy living was the best cure for consumption; that was reflected in Acts of Parliament. He asked the jury to say that there had been adduced before them no evidence that the remedy was a cure, and that the plaintiff had no justification for the statements which he made.

Mr. Tindal Atkinson, replying on the whole case, said the reason there had been delay in bringing the action was that the defendant wanted to have evidence of further experience in the use of the drug. It was for the defendants to prove that the drug was useless. All that their witnesses were able to say was that they were not satisfied that this was a cure. That was not sufficient. It appeared that it was an analyst, not a doctor, who was the libeller. It was for the defendants to prove that it was not a cure, and that the plaintiff knew it was not. After reviewing the evidence, counsel said that the charge of dishonesty had been withdrawn by one of the most eminent witnesses for the defendants. Eminent medical men refused to test the medicine. It was anathema to them. They would have nought to do with it because a layman had invented and was making money from it. The medical profession were always slow to accept these new ideas. He asked for substantial damages.

JUDGE'S SUMMING-UP.

Mr. Justice Pickford, in beginning his summing up, which was very full, said that the case was important, because if the plaintiff had been accused wrongfully of dishonourable conduct it was important for him that he should be cleared; but it was also important that the defendants, representing the medical profession, should not improperly be confined in their criticism of any new remedy that was being sold or pushed among consumptives or other patients. He continued: Something has been said about the invariable tendency of the medical profession to oppose everything. I doubt if any such tendency exists at all. At any rate, I think we may say that the medical profession would be most reprehensible if they were to take up every nostrum, considering the number we know there are in the world, without examination and give it to their patients; and if they think that people are being deceived into spending money and hope for things which are worthless they have a right to say so. Of course, in saying so, they must not go beyond the bounds of what is fair to the person whom they are criticizing. The case is important from that point of view. It does not seem to me that your verdict, whichever way it goes, necessarily involves a pronouncement as to whether this is a good and valuable remedy for consumption or not. I doubt very much, with all respect to you—I can speak for myself—whether you and I would be a very satisfactory tribunal to decide such a thing. Probably a suggestion made to me by a distinguished French professor that such a matter as that ought to be decided by independent medical or chemical gentlemen is most valuable, but it does not seem to me really that it is necessarily involved in your verdict. I have not the slightest doubt that whichever way your verdict goes it will be treated by one side or the other as determining that question, but that question is not involved at all. What the plaintiff says is this: "You say of me that I am fraudulently and deceitfully pretending that I have a good remedy, and that I know it is not a good remedy, and that therefore, with full knowledge that I am deceiving the public, I am putting this remedy before them." That is his complaint, put shortly. He says that they say of him that he is knowingly putting an absolutely worthless thing before the public and working upon the fears of people in the last stage of consumption in order to put a worthless thing before them. It might very well be that he is entitled to a verdict, even although you thought this was not a thing of any use at all, if he honestly believed that it was, because the accusation against him from his point of view is that he has knowingly put a worthless thing before the public. Although it may be of no value, if he honestly thought it was and said nothing more than what he honestly thought, this would be a libel upon him which would not be justified. In that case, whether it be good or bad, he would be entitled to your verdict if you were to take that view of the evidence. On the other hand, what the defendants say, as I understand their case, is this: "We did not go as far as that, but what we did say and what we do say is that, looking upon your system of putting this thing before the public, looking at the advertisements, and looking at what you claim for this remedy in those advertisements, you were claiming that this remedy could do what you knew it could not do. Although it might do something, you were claiming that it would cure cases in a very advanced stage of the disease when you knew quite well, and indeed have admitted in the witness box, that it could not cure those advanced cases." If that is so, and if they prove that, they might be entitled, and probably would be entitled, to your verdict, although you might think there was some value in the drug. That is the first defence. Then, as a second line of defence, they say that, even supposing they have not actually proved that to be the case, at any rate the plaintiff's conduct and his method of advertising and the wording of his circulars was such that what they said was only fair comment upon those circulars, and, therefore, was comment which they were entitled to make. To support a verdict for them it is not necessary that you should find that this is a worthless thing. There may be some value in it, although the plaintiff might, for the purpose of making money, have claimed more for it than it could do. Therefore it does not seem to me that your finding necessarily involves whether this is a good remedy for consumption or not. In considering the case you must not allow yourselves to be run away with by words that have ambiguous meanings. A good many words have been used, and necessarily used, in this case. I refer to such words as "efficacy," "cure," "swindler," and so on. A thing may be efficacious up to a point, but if you say it is efficacious a long way beyond that point when you know it is not, then it is not efficacious in that sense. I need not give you an instance of "cure," because I think most of the witnesses have said that "cure" is used in different senses by different people. When you come to the word "swindler," or anything of that kind, you must look at what kind of a swindler he is accused of being, if he is accused of being a swindler at all. It is dangerous to jump to a conclusion by taking one of those words without stopping to think in what sense it is used, and how it ought to be applied in this case.

His Lordship then reviewed plaintiff's history down to the time when he commenced to advertise in this country, quoting largely from the advertisements and circular letters. In considering the bearing of the defendants' article, and whether the defendants had justified it, either as true or as fair comment, the judge said that what the jury would have to say about it depended in a great measure upon the view they took of the circulars. If the jury considered that these circulars, although perhaps in exaggerated language, only fairly represented that this was a good thing that people ought to take, he, the learned judge, did not know that there would be any harm in it, or that the defendants could justify the comments they made; but if the jury thought the advertisements were intended to convey to persons suffering from this disease, and to persons especially of no great education and not much medical knowledge, that no matter how bad the disease was, short, of course, of being on the actual point of death, this was a cure, and in substance the only cure and the absolute and certain cure, then it would be a question for the jury whether they did not honestly and fairly represent what he had said in evidence was his belief at the time of the effect of this drug. There were certainly expressions about the third and last stage, about advanced stage, and about persons who had been given up by sanatorium doctors, and they might bear that construction. If the jury thought that was the construction to be put upon them, then it could hardly have been done for any other purpose than the sale of the medicine, and the jury must consider whether the extent to which he believed this remedy was efficacious was fairly and honestly expressed. It was necessary to distinguish and consider in what sense the words were used. A man might think that in a very early stage the remedy might be of some use. He did not know where or how. The plaintiff said in one of the advertisements that it absolutely killed the bacilli, but that the learned judge supposed was only a conclusion from the fact that the disease went. He did not know where or how, but he might honestly believe that, taken at a certain time and up to a certain point, the medicine might be useful, but he might not believe, and have no reason whatever to believe it, when applied to any case however advanced, and to a case given up by every doctor and by every sanatorium in the country. Which was the right construction to put upon the circulars and documents was one of the important matters the jury had to consider. The learned judge then read extracts from the article, and quoted the results of the analysis:

"The medicine was a clear red liquid, and analysis showed it to contain, in 100 fluid parts, 21.3 fluid parts of alcohol, 1.8 parts of glycerine, and 4 parts of solid substance; this solid substance contained about 1 part of tannin and 0.2 part of ash, the remainder being extractive. No alkaloid was present, and no other active substance could be detected. The solid substance agreed in all respects with the solids of decoction of krameria, or a mixture of this decoction with a little tincture of kino. The formula thus appears to be approximately rectified spirit of wine 23.7 parts by measure, glycerine 1.8 parts, decoction of krameria (1 in 3) to 100 parts by measure, or it may be made with tincture of krameria. Estimated cost of ingredients for 2½ fluid oz., 1½d."

Now, gentlemen, it has been said that that is the foundation for the whole of this libel, and that, when it is once displaced and once shown that this stuff is not made of krameria, there is practically an end of the case. I confess it does not strike me exactly in that way. This is an analysis which may or may not be correct. Mr. Harrison still says it is a correct analysis of what he got in 1908. It may be that Mr. Harrison was wrong in his conclusion that this was krameria and kino; it may be

that there was something else; it may be that it was umckaloabo or chijitse. But that is not the whole foundation of the article. The foundation of the article is that Mr. Stevens is claiming for this something which he knows that he cannot perform. Mr. Harrison says he does not believe in it because he believes it to be krameria. He does not say that in so many words, but, assuming that is what he does say, and supposing it is shown that he was wrong, that it was not krameria, it takes some of the foundation upon which he formed his conclusion away. But whether Mr. Harrison was right or wrong, further analysis has not revealed, as far as analysis can reveal, anything in umckaloabo or chijitse, so far as its qualities in dealing with tuberculosis go, that there is not in krameria. I do not say, mind you, that because analysis cannot find it it is not there. I am not putting that before you. It seems to me that this discussion about the analysis does not come to very much, in face of the fact that the analysis which has been made, although it may be shown negatively that Mr. Harrison was wrong, does not show positively that there was anything in these two roots which could deal with the bacillus or the disease of tuberculosis. If the analysts had been able to say: "Yes, we have found something which showed a chemical result or an analytical result which convinces us that this is a germicide or something which will deal with tuberculosis," then this conflict about the analysis would have been very much more important than it is now. The analysts called on behalf of the plaintiff were, first, Dr. Hehner, then Mr. Dalkin, and, in a lesser degree, Mr. Lord. I say in a lesser degree Mr. Lord, first, because he is a bacteriologist and not a chemist, and secondly, because since 1908 he has been, I think, in Mr. Stevens's employment. I do not say cast him out altogether, but I do say he is certainly not, to my mind, so important a witness as Dr. Hehner and Mr. Dalkin. There was some question about Mr. Dalkin as to the way he got his sample. That comes to nothing; it was given to him in a straightforward manner. There is nothing to be said against Mr. Dalkin. I do not know which analysis is the right one. It may be that Mr. Harrison is wrong in his analysis. He does not say it was krameria, but he says he does not know of anything else that is more like it. If he was wrong, then some part of the foundation of this article may have gone. In my opinion it does not go further than that. If it does go further than that in yours you will say so. Now, dealing with the rest of the article, it undoubtedly does say that Mr. Stevens is knowingly claiming for this drug a power and effect which he knows it has not got; that he is claiming that it will cure even in the very worst cases, in the third and last stage, when he knows it will not. It does undoubtedly do that. The defendants say that is all it does. The plaintiff says, No, it does more than that. It not only says that, but it says that he, Stevens, is knowingly claiming this for something which he knows is no use at all. Which of those meanings is to be attached to it is a matter for you to say, reading it over. Whichever the meaning is, have the defendants proved it? That is the first question. They must prove that it is true. I should think you would be of opinion that, whichever is the meaning, it is a defamatory article, because it is saying, Whether it is worth nothing or whether it is worth something you are putting it before these unfortunate people as something which will cure any case—in fact, as almost the only cure, and you are doing that to induce them to come to you and pay money. Now have the defendants proved that? Part of that must depend on what you think of the circulars themselves, and part must depend upon the other evidence as to what was before Mr. Stevens at the time.

Now I do not intend to go into every single case that has been put before you, but I shall have to go into some of them in order to see what the effect is. Broadly stated, it seems to me the defendants' case with regard to that is this: There is nothing in any one of those cases that cannot be clearly referred to something altogether apart from the plaintiff's cure. One of the doctors called on behalf of the defendants—I think it was Dr. Latham—put it very clearly: In order to see that a drug is really doing good, and that it has an effect of itself, you must, in a sufficient number of cases, be able to say that those cases show some different cause, run a different course, and act in a different way to those that come under ordinary treatment. If you do not find that, he says, you cannot say it is the drug that is doing it, because you have ordinary treatment at the same time. That is what I gather from what he said.

Now, gentlemen, you have had a number of cases put before you. First of all we had Dr. Grün, then Dr. Naylor, then Dr. Campbell, then Dr. Evans, then Dr. Sames, and then Dr. Deboissiere. It is important, I think, to see the conclusion that those doctors came to. Dr. Grün said he considered it had a specific effect—that is to say, not only an effect upon the symptoms, the cough and so on, but a specific effect upon the disease. He also said he could not say it would cure all cases. Dr. Naylor said he did not consider the medicine an entire cure from what he had seen. Dr. Campbell said: "To say the medicine would cure all cases is too strong a statement"; and he also added that hygienic conditions and climate were in his opinion more important than any drug. Dr. Evans said he could not speak with certainty to its efficiency yet. You will remember that he was the gentleman who tried it upon the greatest number of cases—twenty. I do not think Dr. Sames or Dr. Deboissiere expressed any opinion of that kind. Now those are the conclusions to which the doctors came, and they came to those conclusions upon the cases which they told you about. Of Dr. Grün's cases the first was the case of a patient who was not cured: he said he was better, but not cured. The second case was the case of the boy at Shoreham. He said there was a cavity in the lung, that there was tubercle bacilli in the sputum, that he gave him the drug, and at the end of three months there was no mischief to be found in the lung and no bacilli. The third case was the case of the patient named Cross who has been called before us. He was certified by four well-known doctors to have tubercle bacilli. That is, of course, what the patient told Stevens, and, as far as it goes to bonâ fides, the fact that Mr. Stevens was told so, is a matter to be considered, but the fact that the man said so does not prove that he was so certified; and when he was asked what was told him he does not mention tuberculosis at all. He said: "He told me he was suffering from bronchitis, inflammation of the lungs, and pneumonia." Then the fourth case was the case of a man named Hersee, a gardener, who died. He is said to have died from pneumonia. As far as I remember we have no evidence at all, not even a certificate, as to what he died from; but he died, and therefore one cannot draw conclusions from that.

Then Dr. Naylor was the next. He gave four cases, and in each of those he says quite clearly that in his opinion this drug did good. The first case he does not suggest to be cured. It is still at a standstill, as I understand it. The second is a case which, perhaps, you ought to consider, and consider it with another case—that of the man Day. Dr. Naylor's second case was the case of a patient who had been treated in the ordinary way—sanatorium treatment, hospital treatment, open air treatment, and so on—but who had got a very bad cavity in the lung, and a very bad place in the lung. Day was the man who had an abscess in the lung and another abscess in the testicle. In the one case, Dr. Naylor's second case, after taking the medicine, a great deal of the lung was thrown up, there was a very bad fit of coughing, and a great deal of the lung was thrown up. In the other case, the abscess burst externally, and that was before the plaintiff's medicine was taken. After the throwing up of the lung, in the one case, and after the bursting of the abscess in the other case, the patients recovered. According to all the medical men there are cases—they are rare, but they do happen—in which, if the whole of the diseased part of the lung is thrown up, or if the whole of the abscess, either by operation or by bursting, is removed, the patient recovers; and what is said about those two cases, which are two of the most striking cases, is that that is the sort of thing that happens, and that they are no proof that Mr. Stevens's drug had anything in the world to do with it. The doctors who were in attendance—each of them—think it had. They think that Mr. Stevens's remedy had something to do with it, and that in one case it was the cause of it, and in the other case the doctor said he could not say positively whether it was the cause of it or not. Now it has been said that the doctor attending the patient can, of course, tell better than any specialist who only hears a record of the case. Given the same qualities in each man, no doubt that is true; but even without so intimate a knowledge a specialist may be just as well able to judge. That is the reason you go to a specialist, in order to get his special knowledge in addition to the man who is attending upon you. You must take it into consideration that the doctors who were attending on these people say that in their opinion this medicine had a beneficial effect, and what is perhaps of more importance in this case they reported it to Mr. Stevens, and if they thought so and told Mr. Stevens so Mr. Stevens would have a right to think so too. Then Dr. Naylor's next case was a case in which there was surgical treatment. What is said about that is that the surgical treatment may have been the cause of the amelioration, and not Mr. Stevens's drug. The other case was a case in which there still remain enlarged glands. Dr. Naylor thought they were due to tonsillitis, but I suppose no one could be absolutely positive as to whether they were due to tonsillitis or to a continuation of the disease in the system. Dr. Campbell was the next. He says that in his opinion, in the cases he treated, this mixture undoubtedly did do good. He says the cases treated with the plaintiff's medicine improved more rapidly than other cases, otherwise the improvement was the same; it was somewhat more rapid, but it was otherwise the same. He also said what I have already told you, that he ascribes more to good climate and hygienic conditions than to the action of the drug; and he also said: "To say the medicine would cure extreme cases is too strong a statement; it cannot be done, and to say that would be wrong." Dr. Hudson Evans also gave a number of cases—twenty, I think. I do not think it is necessary for me to go through them all, and for this reason—the summing-up of them all is that he believes sufficiently in the efficacy of this mixture to go on and use it further, but he cannot speak with certainty as to its efficacy yet. Dr. Sames spoke of Day. I have already dealt with Day's case. I do not think Dr. Deboissiere said anything that makes it necessary for me to call your attention to anything in detail.

In addition to those, you have the cases of Mrs. Whiteman, of Miss Bohm, of Miss Olley, and of Mr. Hodgson. Considering the way those cases have been commented upon, I do not think it is necessary for me to go into them at any length. You have heard the suggested explanations; you have heard the evidence of the patients themselves; and you have also heard in some cases the evidence of the doctors who attended them; and you will be able to form your opinion as to whether they do or do not prove the value, and to what extent they prove the value, of this drug.

Now, in opposition to those cases several expert gentlemen have been called on behalf of the defendants—Dr. Acland, Sir

Richard Douglas Powell, Dr. Price, and Dr. Latham. They are very eminent gentlemen. You have heard their comments upon these cases, and you have also heard their cross-examination. With regard to Dr. Acland's cross-examination I have just one word to say. Dr. Acland said—and was sorry for having said it, and corrected it—that if these doctors who had been called for the plaintiff still continued in the opinion that this was a cure for consumption, he thought they must be dishonest. He withdrew that, and he said he thought they must be mistaken, and that a man might honestly be mistaken. What particular belief of the doctors he had in his mind at that time I do not know, and it is for that reason that I think you have to look carefully at exactly what these doctors did say. If the plaintiff was representing that he could cure any case of tuberculosis, even in an advanced stage, what he calls the third stage, and that he could make practically certain of curing those cases, I do not think those doctors would have expressed that opinion. They have expressed the opinion that this stuff is of value. It may be that it is of value; it may be that it is not; but I do not think any of those doctors expressed the opinion that it would be right to put this before the public, and before those persons suffering from consumption, as a medicine that would be practically certain—I do not say absolutely certain, because any case may go wrong—to cure consumption even in the third stage when the patients have been given up by other doctors, and by sanatoriums. And when Dr. Acland says that the testimonials which the plaintiff got certainly afforded material to believe in the efficacy of his remedy, again I say you must consider "efficacy to what extent." As Dr. Acland says, I should think you could hardly deny that, after all these testimonials which the plaintiff received from medical men, he must have believed that it was a good thing. He may be wrong. It may be that it only allays symptoms and does not touch the real disease at all. He may be wrong, but I should think it is very difficult to say that he had not material before him upon which he could honestly come to the conclusion that it was doing good in some cases and was of value up to a certain point. It is also important to consider whether those reports that he had from the doctors, and the reports he had from the cases, would lead him to the belief, if he did believe it, that his medicine would cure in the sense that I have put before you, that is to say, in the sense of curing with practical certainty advanced cases, even in the last stage. If you think that in these circulars and letters he did represent that it would cure such cases with practical certainty, then you have to consider whether he believed that, or whether he had reasonable material upon which he could honestly come to the conclusion that the cure was as good as that.

Now, gentlemen, I do not think it is necessary to go through any more of the facts with you. Whether you take the defendants' construction of this article or whether you take the more extended construction that the plaintiff asks you to take, I should think there can be no doubt that it is defamatory, whichever construction you take, because on the defendants' case it is saying: You were dishonestly telling people that this stuff would practically cure every case, however advanced, when you knew it would not. If he was doing that he was dishonest. If you take the more extended view that the plaintiff asks you to take, it is this: "You knew not only that this would not cure advanced cases, but you knew it was wholly valueless when you were telling them that it would cure consumption." If it means that, then it is a still greater aspersion. If it is an aspersion, the defendants, in the first place, say it is true. That they have to prove. I do not say it is necessary for them to prove it by the evidence which they have called alone. It is an answer if they prove it on a review of the whole of the evidence. The onus is upon them to prove it, and they must prove it either by cross-examination of the plaintiff's witnesses, or by calling witnesses of their own, or by both. If they have proved it there is an end of the matter. If they have proved that in substance Mr. Stevens was knowingly putting before the public, and before those suffering from consumption, claims in respect of this medicine which he could not in the slightest degree believe, and that it was done for the purpose of getting money, then, as it seems to me, they would have substantially proved what is stated here. Of course they would not prove perhaps every word; but they would also, of course, have to prove that he set out consciously and deliberately to deceive. If they have proved that he did what I have said, then probably you would be of opinion that they have proved that also. At any rate it is for you to say, first, what is the defamatory meaning conveyed in this article, and, secondly, have they proved it? If they have substantially proved it, then they are entitled to your verdict. If they have not proved it, then there comes another question. They say: "Even supposing we have not been able to convince you, the jury, that what we said is all true, all proved, still we are entitled to your verdict if we did this honestly as fair comment upon a matter of public interest." That question has not been discussed, and there is no doubt about it. Then the question will be: Are the comments that they made fair? It has been laid down more than once that a comment cannot be fair if it imputes improper motives. It is quite established not to be correct now. If the facts upon which you are commenting are such that they fairly give rise to an inference of dishonest motives, then if you honestly write about them and do not exceed the inference that can be fairly drawn, you are protected, even though that inference might convey an imputation. But there are two things to be considered: First, you must not mix up the facts and the comment in such a way that nobody can tell what is fact and what is comment; and, secondly, the facts upon which you comment must be correct. You must not invent facts or misstate facts, whether negligently or not. You must not incorrectly state facts, and then upon these incorrect facts comment so as to impute improper motives or improper conduct to anybody else. It has been quite clearly laid down by the House of Lords that that is so; and what is said by one of the learned lords is this: "To say that because a comment contains a personal attack it cannot be fair, is wrong. A personal attack may form part of a fair comment upon given facts truly stated if it be warranted by those facts, in other words, in my view, if it is a reasonable inference from those facts." Therefore, if the defendants here have correctly stated the facts upon which they are commenting, if they have fairly and honestly commented upon those facts without any spite or ill feeling or other bad motive, then they will be protected, even although they have not been able to prove every word of what they have said, because they would only have commented fairly upon what is a matter of public interest. The two things are very near one another in this case. In my opinion it is very difficult to distinguish between them, because if the plaintiff did make improper claims for this medicine which he knew to be improper for the purpose of getting money, then it seems to me that the defendants would have proved practically what they said, subject to the question whether what they have written meant something more than that; and, on the other hand, if the plaintiff did not do that, then to say that he did would be misstating the facts. Therefore I cannot see that in this case there is very much distinction between the two. But you have these two matters to consider: First, have they proved it is true? If it is true, you need not trouble about fair comment. Secondly, if you do not think it is true, then have they fairly and honestly commented upon a correct state of facts? If they have, they would be protected, even though they have not proved exactly what they have said.

Now, if you think the defendants are entitled to a verdict, of course there is an end of the matter, and you need not bother about damages. If you think they are not entitled to a verdict, then the plaintiff is entitled to damages, and the question will be, what damages? He has not claimed any special damage, that is to say, any actual money loss. But if the defendants have imputed to him dishonest conduct in the way that has been described so often both by the learned counsel and by myself, I do not think we differ very much about the kind of dishonest conduct that is imputed; and if there is no reason or ground for it, and there is no sort of aspersion upon his conduct at all, then you would give him not extravagant but substantial damages—considerable damages—because it is an imputation of dishonesty, an imputation that ought not to be made unless there are grounds for making it. If you think that they have not justified entirely what they have said, but that the plaintiff's conduct and the way he behaved with regard to this remedy was such that he brought it a good deal upon himself, that would be a matter which you might be entitled to consider in the damages, and then you probably would not give him so much. I do not know that I can give you any further direction about the damages. If you think there is no ground for this attack at all, the damages should be substantial; on the other hand, if you think that, although it is not proved, his conduct has been such as in a great measure to bring the matter upon himself, then you will take that into consideration in the reduction of damages.

After some discussion with counsel, the learned judge further told the jury that it was for the defendants to justify all their observations. The justification of part only would not be sufficient.

The jury then retired, and after nearly two hours' consideration returned to the court, when the foreman stated that there was no possibility of agreeing, and the judge said that there was no alternative but to discharge them.

ACTION FOR WRONGOUS DISMISSAL AND SLANDER AGAINST AN ASYLUM SUPERINTENDENT.

SHERIFF-PRINCIPAL MACKENZIE has dismissed an appeal for wrongous dismissal and slander raised by Malcolm John Monk, late deputy head attendant in Inverness District Asylum, against Dr. Mackenzie, medical superintendent of the institution. Pursuer claimed £300 in name of damages. The slander, it was stated, was written in a certificate given to pursuer. His lordship held that from the rules governing the institution and employees, the defender had power to dismiss pursuer either for assault on a patient, which was the reason assigned for his dismissal in the leaving certificate, or for failing to report at the earliest opportunity that he had been obliged to use force towards a patient, which was alleged by pursuer to be the ground on which he was, in fact, dismissed. In view of the public position which the defender occupies, his lordship was of opinion that pursuer could not be allowed a proof in support of his claim unless he not only averred that defender acted maliciously, but that he set forth facts and circumstances from which malice might be inferred. The statement in the leaving certificate was privileged. His lordship awarded expenses to defender.

A CHARGE DISMISSED.

BEFORE the Axminster magistrates on October 29th a charge was brought against Dr. B. G. Pullin by Mr. J. R. M. Mallett, of Seaton, of committing perjury in a county court action heard twelve months ago at Axminster. The statements were in regard to a *post-mortem* examination on complainant's child, respecting which Mr. Mallett had alleged professional

negligence against Dr. Tonge, of Seaton. It was now alleged that Dr. Pullin stated that the brain at the *post-mortem* examination was not examined. Dr. Pullin was also alleged to have said that he was never asked whether Dr. Tonge was right in his diagnosis. It was complainant's case that both these statements were false. Among the witnesses examined were the shorthand writers present at the county court action. At the close of the complainant's case it was submitted that there was no evidence to go to a jury, and the magistrates, without hearing witnesses for the defence, dismissed the case. Mr. Schultess Young (for complainant) said he would apply to be bound over under the Vexatious Offenders Act to prefer a bill of indictment against Dr. Pullin at the assizes. Mr. J. A. Hawke (for defendant): By all means, let him. I don't know that you can prevent him.

CLAIM FOR NEGLIGENCE AGAINST A DENTIST.

ON October 17th the Sheriff-Substitute for Glasgow gave judgement in an action for damages brought against a dentist. The allegation was that while extracting some of the plaintiff's teeth under gas the defendant had exhibited negligence and want of skill, which had resulted in a tooth or part of a tooth falling into the plaintiff's larynx and thence into her lung, subsequently setting up a grave and prolonged illness, accompanied by blood spitting. The operation was performed on November 25th, 1911, and the plaintiff fell ill immediately afterwards; the doctor who attended her had difficulty in diagnosing the symptoms, which, he stated, had not suggested to him the idea of a foreign body having found its way into the lung. On January 3rd, 1912—that is, thirty-eight days after the operation—the plaintiff, according to her statement, coughed up with some blood a piece of tooth, which she threw away, after showing it to a friend. A month later she coughed up more blood, and had done so at intervals ever since up to the time of the action. Her sputum contained no tubercle bacilli.

The Court held that there was no proof that the plaintiff's illness was due to the presence of a foreign body, such as a tooth, in her lung, or that any tooth or piece of tooth had ever fallen into her larynx. The fact that such accident had occurred was merely a supposition, while there was no proof whatever of any negligence or want of skill on the part of the defendant. On the contrary, what evidence there was as to the precise facts of the operation supported the belief that all usual and necessary precautions had been taken during the operation, and that no tooth nor part of a tooth had been allowed to fall into the plaintiff's throat, and judgement for the defendant, with costs, was returned.

The defendant was represented by Messrs. Turnbull and Findlay, acting on the instruction of the Medical and Dental Defence Union of Scotland.

Universities and Colleges.

UNIVERSITY OF OXFORD.

Degrees.

THE following degrees have been conferred:

D.M.—A. R. Wilson.
B.M., B.Ch.—J. L. Birley.

Medical Fellowship.

Mr. H. C. Bagett, B.M., B.Ch., has been elected to a medical fellowship at Magdalen College.

UNIVERSITY OF LONDON.

Faculty of Medicine.

A MEETING of the Faculty was held at the University on Friday, November 1st. The Dean (Dr. Sidney H. C. Martin) was in the chair.

Dr. Reginald Cecil Wall, M.A., M.D., F.R.C.P., was elected Secretary to the Faculty.

The constitution for 1913 of the Boards of Studies with which the Faculty is concerned was decided; as was also the constitution of the Board of the Faculty for the remainder of 1912.

A report of the Board of Studies in Hygiene and Public Health for the provision in London for the post-graduate teaching of these subjects was presented, and, after some discussion, referred to the Board of the Faculty for report.

Sir Alfred Pearce Gould, K.C.V.O., M.S., was unanimously elected Dean of the Faculty for the period 1912-14.

NATIONAL UNIVERSITY OF IRELAND.

AT a meeting of the Senate on Wednesday, October 30th, Sir Christopher Nixon was reappointed Vice-Chancellor of the University, and representative of the University on the General Medical Council.

A Travelling Studentship of £200 a year, tenable for two years, in Medicine (Anatomy), was awarded to Henry L. Barneville, B.A., M.B., B.Ch., B.A.O., University College, Dublin.

UNIVERSITY COLLEGE, CORK.

President's Report.

THE report of the President of University College, Cork, for 1911-12 states that the number of students during this session was 430, as against 415 during the session 1910-11. Of the 430 students, 358 were matriculated, 48 were non-matriculated, and 23 were post-graduate. The number of new students was 130. The work done during the session was of a satisfactory character.

The college had sustained a grave loss in the death of Professor George Laurence, the first occupant of the Chair of Law of Real Property. Though retired from the active duties of the Chair of Physiology and Anatomy, the news of Professor Charles's death had been received in University College with sorrow, his connexion with the college dating back to 1875. During a period of thirty-two years Professor Charles filled his professorship with great distinction, and on his retirement founded an annual gold medal in the college, to which his name is attached.

The President, in referring to the different faculties, draws attention in particular to that of the engineering school, the new laboratories of which are now almost complete, and contains an installation of apparatus which is unrivalled in the country, particularly in the testing machines of various kinds. A very large amount of public work was carried on in the engineering laboratory with the aid of this equipment.

Perhaps one of the most notable advances made by the college in the past year was the taking over of the old Cork football grounds, now known as the University Athletic Grounds. The President referred to two very necessary requirements if the college is to go on to further developments. The first, which he declares is essential, is to obtain separate university powers for Cork. He hopes "the governing body will not be wearied by my perpetual insistence on this matter, but it is the most urgent question in connexion with the college, and the moment that a favourable opportunity arises no time should be lost in making a determined effort to secure the boon which we have every right to demand." The second necessity was to raise by subscription from the citizens of Cork a fund sufficient to purchase the freehold of the various portions of land on which rent has at present to be paid.

The report closes with a reference to the harmonious relationships which have existed between the academic and non-academic members of the first governing body and the president himself. "We have had a very arduous task laid upon us; it was one which necessarily gave rise to many differences of opinion, but none of them have in the least strained the feelings of friendship which have existed from the beginning between the members of the governing body, the Academic Council, and myself."

ROYAL COLLEGE OF PHYSICIANS OF LONDON.

A COMITIA was held on Thursday, October 31st, Sir Thomas Barlow, Bart., K.C.V.O., President, being in the chair.

Admission of Members.

The following gentlemen, having passed the required examination, were admitted Members of the College: John Davis Barris, M.B.Camb., L.R.C.P., and Ardeshir Koyaji Contractor, M.D.Lond., L.R.C.P.

Licences.

Licences to practise physic were granted to ninety-five gentlemen who had passed the necessary examinations.

Diploma in Tropical Medicine.

In conjunction with the Royal College of Surgeons of England, the diploma in Tropical Medicine was granted to Dr. E. S. Marshall.

Announcements.

The President announced that the Weber-Parkes Prize for 1912 had been awarded to Mr. John Alexander Douglas Radcliffe, M.B., Pathologist to the King Edward VII Sanatorium, Midhurst. He also announced that the adjudicators for the next essay in 1915 were: Sir J. Kingston Fowler, K.C.V.O., Dr. F. J. Wethered, and Mr. L. S. Dudgeon, F.R.C.P. The subject chosen was: An original research on the treatment of pulmonary tuberculosis with substances which are especially antagonistic to the specific organism and its products. This work must have been chiefly carried out since the year 1911.

Communications.

The following communications were received: (1) From the Secretary of the Royal College of Surgeons of England, dated July 25th and October 22nd, reporting proceedings of the council of that college at meetings held on July 25th and October 11th. (2) From the University of Dublin dated August, 1912, thanking the college for sending delegates to the bicentenary celebration and for presenting an address on that occasion. (3) From the Curator of the City and County Museum, Lincoln, thanking the College for presenting to the Museum the collection of antiquities made by the late Dr. O'Neill.

Discussion on Sanatorium Benefit.

Dr. W. J. Tyson, speaking with reference to sanatorium benefit under the Insurance Act, called attention to the great importance of associating the treatment of tuberculosis in all its phases with the existing hospitals, in order that medical students may have every access to the study of the disease and

its treatment. He referred to the letter written by Sir William Osler to the *Times*, and to a leading article which had appeared in the BRITISH MEDICAL JOURNAL. He said that schemes for working the benefit were been propounded in numerous districts, but the subject of the special education of medical practitioners and students in regard to tuberculosis had not been fully discussed. Pulmonary tuberculosis was likely to be dissociated from general hospitals and medical schools. The study of the disease ought to be linked up with the schools, and the general hospitals should have beds for patients suffering from tuberculosis. He thought the college should take the lead in drawing attention to the danger of students passing through the schools without opportunities of studying the disease. An appeal ought to be made to the Government to see that this great question is put on a proper basis. Dr. Tyson explained the scheme for the County of Kent.

Dr. H. P. Hawkins stated that the authorities of St. Thomas's Hospital had decided to form a central institution for tuberculosis, in order that students might study the cases. A dispensary separate from the hospital had been started with a whole-time officer. It was practically a new institution for the treatment of tuberculosis in the Borough of Lambeth.

Dr. H. W. G. Mackenzie said that if tuberculous patients were withdrawn from the general hospitals students would have to go to the special hospitals if some other arrangements were not made. Tuberculosis had to be studied in connexion with other morbid conditions of the body. As subscriptions to the chest hospitals were falling off, they seemed likely to perish from inanition. These hospitals had rendered great services to the public, and if they were closed a great means of combating the disease would be lost. He explained the measures which were to be adopted at the Brompton Hospital both in regard to the treatment of patients in association with the surrounding boroughs and with regard to a teaching scheme whereby practitioners and students could obtain tuition for from three to six months.

Sir Richard Douglas Powell pointed out that as at present arranged the treatment of tuberculous patients was to be carried out by "specialists" in various districts, and patients were to be drafted into sanatoriums. He would greatly regret if the patients were taken out of the hands of general practitioners. He laid stress on the necessity of studying pulmonary tuberculosis in connexion with general diseases, but up to the present no proper provision had been made for tuberculous patients at general hospitals. He would like to see general practitioners told off to attend patients in small sanatoriums. In large cities resident medical officers would be necessary, but a visiting staff should also be attached. In the first instance, however, practitioners must be properly educated with regard to tuberculous disease. A central bureau should be established for the scientific study of tuberculosis and for periodically distributing the knowledge acquired.

Dr. C. Theodore Williams drew attention to the unsatisfactory arrangements made in connexion with some of the dispensaries which had been scattered through the country. He strongly advocated keeping the dispensaries in connexion with existing large hospitals.

Dr. W. C. Wilkinson drew attention to the financial problem of the tuberculosis question, and said that the funds proposed could only under the present scheme supply treatment to a limited number of patients. He thought that dispensaries should be mainly for treatment and not for prophylactic purposes.

Dr. Norman Moore also urged that the profession was now concerned with a movement which might greatly affect the teaching at large hospitals, and expressed the hope that the College would appoint a committee to consider the matter.

Dr. F. E. Fremantle referred to the appointment of medical officers of health as tuberculosis officers, who were asked to advise as to the steps which should be taken in their districts. Those officers were isolated from the profession in that they had to give advice to lay bodies. The College could carry great weight with county councils. He also referred to the endowment for research contained in the Act.

The President urged that the Government and the public should be enlightened on the subject. If a committee were appointed he thought that the whole subject of the sanatorium benefit should be considered.

Dr. David Finlay proposed that the whole subject should be referred to the committee which had been previously appointed in the College to deal with the Insurance Act, with the addition of Dr. Tyson. This was seconded by Dr. Tirard and carried.

Reports.

In accordance with reports dated October 8th received from the Committee of Management, the Leamington Spa Municipal Technical School and the King William School, Castletown, Isle of Man, were added to the list of institutions recognized by the Examining Board in England for instruction in chemistry and physics, and the West Bromwich Municipal Secondary School and King Edward's School, Birmingham, for instruction in biology. The Naval Medical College at Greenwich was added to the list of laboratories recognized for the course of instruction for the D.P.H., and the East Ham Borough Isolation Hospital to the list of recognized fever hospitals; the course of instruction in lunacy by Dr. E. D. Macnamara at Peckham House was recognized as fulfilling the conditions of the regulations of the Examining Board in England.

Committee of Management.

Dr. Norman Moore was re-elected a member of the Committee of Management.

Library.

Books and other publications presented to the library during the past quarter were received, and the thanks of the College ordered to be returned to the donors. The Harveian Librarian (Dr. Norman Moore) announced that the President had presented some letters of Harvey. A most hearty vote of thanks was accorded to the President for this splendid and uncommon gift, and, as a special mark of the appreciation of the Fellows, the thanks were given under the College seal.

After some further formal business, the President dissolved the comitia.

ROYAL COLLEGE OF SURGEONS OF ENGLAND.

THE ANNUAL REPORT.

THE annual report of the Council for the year 1912, which will be presented to the meeting of the Fellows and Members at the Royal College of Surgeons, Lincoln's Inn Fields, on Thursday, November 21st, at 3 p.m., has been issued. The agenda of the meeting will be issued on November 18th, and copies may be obtained by Fellows or Members on application to the Secretary.

RECORD OF THE YEAR'S WORK.

At the annual meeting last year four resolutions were passed. The first referred to the resignation of the late Sir Henry Butlin; the second to the attitude assumed by the Council of the College toward the Chancellor of the Exchequer in regard to the National Insurance Bill; the third dealt with the admission of Members to the Council; and the fourth required a balance sheet of assets and liabilities to be included in the next annual report of the Council. The Council decided not to take any action in reference to the third resolution, and did not pass any motion by means of which the proposal embodied in the fourth resolution would be carried into effect.

National Insurance Act.

The action of the Council in refusing to send representatives to the meeting which the Insurance Commissioners proposed to hold with representatives of the medical profession on February 2nd, 1912, is recorded. An account is given of the steps taken by the Council, together with the representatives of the Royal College of Physicians, the Medical Faculties of Universities and the Society of Apothecaries, to consider what attitude the medical profession should adopt toward the working of the Act. The action of the Council in relation to the pledge drawn up by the British Medical Association is recorded, and it is recalled that a meeting was held with the staffs of the London hospitals, and that this meeting decided by an overwhelming majority to support this pledge.

The Licence in Dental Surgery.

The consideration of the Board of Examiners in Dental Surgery of the report of the Inspector of the General Medical Council on the final examinations for the Licence in Dental Surgery has led to the issue of a revised synopsis of surgery and surgical pathology, which will come into force on May 1st, 1913.[1] Very little is added to the scope of the examinations, but a more systematic arrangement is adopted.

Dental Education.

A statement was presented by the Council to the Royal Commission on University Education in London, relative to dental education. The opinion was expressed that dentistry was not a subject of such breadth of scope as to justify the award of a university degree. The ordinary dental curriculum should, therefore, not lead to a university degree, and the diploma of the Royal College of Surgeons was satisfactory and sufficient. It was further set out that the abolition of private apprenticeship would be premature at present, but that encouragement should be given to students to take instruction in dental mechanics in the dental laboratory of the hospital they attended.

The Central Midwives Board.

Mr. C. H. Golding-Bird, a member of the Central Midwives Board, had pointed out in his report on the proceedings of the board the difficulties arising from the continued employment of unqualified women under the plea that they are not acting "habitually and for gain." In some instances these women had an arrangement with a doctor who did not attend the case unless specially called, though a retaining fee of 2s. 6d. was paid. Strong action had been suggested for dealing with these cases. It has been decided that the time has arrived when midwives should be required to record the pulse and temperature of the patient systematically. The position of the medical man called in on emergency by a midwife under the National Insurance Act was also referred to.

Prizes.

The Jacksonian Prize for 1911 was not awarded. The John Tomes Prize for the period 1909–1911 was awarded to Mr. Arthur Hopewell-Smith, and the Begley Studentship was given to Mr. Alan Cecil Perry.

Gift.

A marble floor in the entrance hall, museum hall, and corridor has been presented to the College by the widow of a member of the College.

[1] BRITISH MEDICAL JOURNAL, June 22nd, 1912, pp. 1437, 1438.

Finance.

The gross income of the College amounted to £25,840, an increase of £137 over the preceding year. The increase was derived partly from fees for the Licence in Dental Surgery, partly from fees for the diploma in Tropical Medicine, and partly from emoluments for the hiring of rooms at the Examination Hall. On the other side of the balance sheet an increase of £287 was recorded. The total expenditure for the year amounts to £23,769, leaving a balance of income over expenditure of £2,071, as compared with £3,221 for the previous year. The museum claimed an amount of £4,157, which was £242 more than last year and £1,200 more than the average for the ten years ending 1907-8. The chief items are—salaries £2,697, furniture and fittings £519, specimens, glass, etc., £326. The expenses of the library were £94 less than in the previous year. On the other hand, the extraordinary expenditure amounted to £1,661, including £980 for the reconstruction of Rooms VII and VIII.

The sum of £10,125 had been paid on account of the new Examination Hall in Queen Square. The assessment of the new Examination Hall was considerably lower than that of the old building.

Diplomas.

During the year 376 new Members and sixty new Fellows were admitted. In addition, thirty-nine diplomas in public health, nine in tropical medicine, and 100 licences in dental surgery had been granted.

Museum.

Important alterations have been carried out in the museum. The invertebrate specimens have been transferred to a new room (VIII), built where the librarian's house used to stand at the east end of the museum, and the surgical instruments have been placed in a second new room (VII) in the same section. This cleared the space of Room III for specimens illustrating the general principles of pathology. A new department is being arranged dealing with the medico-legal specimens. A large number of important additions to the existing series have been made, and some of the older specimens replaced. The specimens illustrating the growth of bone in the Hunter collection having lost their original colour, Hunter's experiments have been repeated, and new preparations have been set up. A new catalogue of the Gynaecological Section of the Pathological Collection has been completed and the series newly arranged and labelled. Some important material has been added to the Anthropological Series, including crania and skeletons of native Australians, some rare specimens illustrative of the osteology and diseases of the ancient Egyptians, material from Buenos Aires, the New Hebrides, Borneo, etc., and some very valuable skulls and skeletons of the ancient inhabitants of Britain.

CONJOINT BOARD IN SCOTLAND.

THE following candidates have been approved at the examinations indicated:

FIRST COLLEGE.—J. Pearson, W. J. Sweeney, A. M. Morris, Elfrida H. B. Coghill, H. C. A. Haynes.

SECOND COLLEGE.—G. B. Charnock, T. W. Drummond, J. S. A. Rogers, J. J. de Waal, M. Seeraj, G. L. Stanley, J. F. Sweeney, A. C. Taylor, N. A. Martin, W. H. O'Grady, B. C. Haller, W. H. Wray.

THIRD COLLEGE.—S. Swaminathan, N. R. Whitaker, V. H. Wardle, R. M. Paterson, Amabile Caruana, J. E. Kitchen, W. T. M. Sebeta, A. M. Burge, D. S. Graham, J. K. Venables, A. I. Clarke.

FINAL.—A. G. Cowper, J. McFarlane, D. S. Puttanna, N. R. R. Ubhaya, W. Martin, E. M. Marcar, W. Watson, A. F. D'Sousa, C. J. L. Patch, H. Mathewson, W. Lessey, H. S. Dastur, V. C. H. Dearden, J. I. Arnold.

Obituary.

DEPUTY SURGEON-GENERAL JOSEPH CHRISTIAN CORBYN, who died at Cheltenham on October 24th, entered the Bengal Medical Service as Assistant Surgeon on November 24th, 1851, became Surgeon on April 8th, 1864, and Surgeon-Major on November 24th, 1871, retiring, with a step of honorary rank, on January 1st, 1879. He saw much active service in the Indian Mutiny. First he served with the Amritsar column, then accompanied the Kashmir Force under Sir Richard Laurence to Delhi, and took part in the siege and capture of that city. He next served at Jhajjar and Rohtak, and had medical charge of the 3rd Sikh Cavalry at Lucknow, and was present at the final capture of the place. With the same regiment he served in the force under Sir Edward Lugard in the North-West Provinces and Bihar, and shared in the relief of Azamgarh; and later, under Brigadier Douglas, in the pursuit of Koer Sinh, and was present in every action fought by that force. With the same troops he served in the final expulsion of the rebels from the Jagdespur jungles, and the quelling of the disturbances in the Shahabad and Gaya districts. He held the Mutiny medal, with clasps for Delhi and Lucknow.

Medical News.

DR. E. L. MCSHEEHY, a retired surgeon-major of the Army Medical Staff, and the present mayor of Wimbledon, has received and accepted an invitation of the town council to continue in office for a further year.

THE chairman of King Edward VII Hospital at Windsor has received from Prince Christian a cheque for £1,557 odd, the balance of the Windsor Memorial Fund to His late Majesty. The amount is to be retained as an endowment fund.

THE special course of lectures and demonstrations announced to be given at the National Hospital for the Paralysed and Epileptic, Queen Square, by Dr. Kinnier Wilson, Dr. James Collier, and Dr. Hinds Howell, will not be held.

LADY LOWTHER is holding a St. John Ambulance Association class at the British Embassy, Constantinople, the lecturer being Mr. S. Osborn, F.R.C.S., of the St. John's Gate Medical Staff, and the examiner, Dr. Clemow, Physician to the Embassy.

THE annual dinner of the Harveian Society of London will be held at the Hotel Great Central, Marylebone Road, on Thursday, November 14th, at 7 for 7.30 p.m. The chair will be taken by the president, Dr. H. J. Macevoy. The charge for tickets is 8s. 6d., exclusive of wine.

THE Glasgow University Club, London, will dine at the Trocadero Restaurant, Piccadilly Circus, on Friday, November 29th, when Professor Dudley J. Medley, M.A., will take the chair. Members desiring to be present are requested to give notice to the honorary secretaries, 30, Seymour Street, W., as early as possible.

AMONG the papers to be read at the meeting of the Royal Society on Thursday next will be one by Mr. J. W. Cropper, M.B., on the development of a parasite of earthworms; another by Mr. James Thompson on the chemical action of *Bacillus cloacae* (Jordan) on citric and malic acids in the presence and absence of oxygen; and a third by Mr. G. W. Ellis and Mr. J. A. Gardner on the excretion of cholesterol by man when fed on various diets.

THE eighty-seventh Christmas course of juvenile lectures, founded at the Royal Institution in 1826 by Michael Faraday, will be delivered this year by Professor Sir James Dewar, LL.D., D.Sc., Ph.D., F.R.S., Fullerian Professor of Chemistry. The lectures will be experimentally illustrated, and the subjects are as follows: Alchemy, Saturday, December 28th, 1912; Atoms, December 31st; Light, January 2nd, 1913; Clouds, January 4th; Meteorites, January 7th; Frozen Worlds, January 9th. The lecture hour is 3 o'clock.

THERE appear to be some thirteen or fourteen candidates for the appointment of medical officer of health for the City of London, shortly to be vacated by Dr. William Collingridge. They are all of them more or less well-known medical officers of health, six being at work in London, five in charge of large provincial cities, and two in charge of counties. The appointment is primarily in the hands of the Sanitary Committee of the Corporation, which will deal with the applications on November 26th. The candidate finally selected will be expected to take up duty on March 20th, 1913.

THE idea of the memorial to Shakespeare, unveiled in Southwark Cathedral by Sir Sidney Lee on November 4th, originated with a member of the medical profession. Sir Sidney Lee, in the course of his address, remarked that the monument he had just unveiled was the fruit of the energy of Dr. R. W. Leftwich, who combined with his beneficent practice of the healing art a rare love of Shakespeare and of the place in which the company had gathered to do him honour. It had been Dr. Leftwich's wish to erect in Shakespeare's memory a Gothic cenotaph resembling and forming a pendant to the neighbouring tomb of the poet Gower, and, thanks to the generous response made to his appeal by English and Americans alike, this wish had been realized. The memorial, which is the work of Mr. Henry McCarthy, a well-known ecclesiastical sculptor, consists of a semi-recumbent figure of the poet in alabaster enclosed in a Gothic shrine, in front of which are shields bearing the arms of Shakespeare, Bishop Talbot, Canon Thompson (the late rector), and Dr. Leftwich. The short dedicatory service which accompanied the unveiling was preceded by an organ recital, by Mr. Edgar T. Cook, of extracts from the works of Shakespeare's contemporaries, Orlando Gibbons and Byrd, and the prayers were read by the Bishop of Southwark. Amongst those present at the ceremony were the American Ambassador, Sir Thomas

Barlow, Sir Bryan Donkin, Sir Frederick Bridge, Professor Gollancz, Dr. Leftwich, Mr. Edmund Gosse, Sir George and Lady Alexander, Mr. and Mrs. Kendal, and Mr. H. B. Irving.

Public Health AND POOR LAW MEDICAL SERVICES.

REPORTS TO THE LOCAL GOVERNMENT BOARD.

Mallwyd Urban District.—It appears from the report (new series, No. 68) to the Local Government Board of Dr. Morgan J. Rees that the Mallwyd urban district, in the County of Merioneth, though technically urban, is rural in character, for the population of some 750 persons is scattered over an area of about 14,000 acres. It is stated the housing conditions are very unsatisfactory, and that no steps have been taken to put in force the housing regulations of the Housing, Town Planning, etc., Act, 1909. A lack of decent unoccupied houses results in the occupation of dwellings quite unfit to be inhabited. There are no building by-laws, and no supervision is exercised over new houses, with the result that in quite recent years two houses were built back to earth. The water supply is, for the most part from dip-wells, open to contamination in many ways, and even a small supply belonging to the district council is liable to be polluted at the source. Slaughter-houses and dairies and cowsheds are subject to little or no supervision; as a consequence they are kept in a very unsatisfactory condition. The cowsheds especially are dirty and ill kept, and appeared as though they were never cleaned out. Dr. Rees asserts that the district abounds in conditions inimical to the health of the inhabitants, and that little has hitherto been done by the district council to secure removal or amelioration of those conditions. In such a small district, however, the resources are not very great. A penny rate produces less than £9, and the salaries of the medical officer of health (Dr. D. W. Morris) and the inspector of nuisances, even excluding the contribution from the Exchequer grants, absorb an amount nearly equal to a twopenny rate.

Barnstaple Borough.—Dr. S. W. Wheaton's report (new series, No. 69) has special reference to the incidence of typhoid fever in the borough of Barnstaple. Since compulsory notification came into force in the borough in 1896 there has been no single year in which cases of the disease were not notified, the smallest number being 3 in 1896 and 1898, and the largest, 49, in 1911. In only four years were no deaths recorded. During the sixteen years 1896-1911 there were altogether 213 cases and 28 deaths. The population of the borough at the last census was 14,488. Dr. Wheaton states that there are many circumstances in Barnstaple which would favour the spread of typhoid fever when introduced. Some of these are especially related to the polluted condition of the River Taw, at the estuary of which shellfish, principally cockles and mussels, are gathered, and in which bathing opposite the principal sewage outfall is greatly indulged in during the summer months. The possibility of water supplies, milk, or ice-cream being causative factors of the outbreak in 1911 was considered by Dr. Wheaton, who was of opinion that they could all be excluded. In the Derby area, where the disease was most prevalent, the housing conditions are described as very unsatisfactory, the dwellings being old, dilapidated, and damp, with many unpaved or badly paved backyards. Water-closets are universally used in the town, but the pans in many instances were found in a filthy condition and the structure of the closet buildings dilapidated. No doubt was entertained that insanitary conditions generally, including defective drains, were responsible for the spread of the disease, and personal contact was a factor. The isolation hospital accommodation in the borough is very meagre, and consists of the upper portion of the old county gaol, the ground floor of which is used as a storehouse for property belonging to the town council and for housing a steam disinfector. The building projects into the cattle market and has no curtilage. The nurses' rooms and the kitchen all communicate directly with the wards, which themselves are intercommunicating.

PAYMENT OF VACCINATION AND NOTIFICATION FEES.

H.S.—Under the contract which every public vaccinator has with the guardians, he is required to make out an account at the end of every quarter of the sums payable to him. If, however, the public vaccinator fails to send in such account at the right time, and six months elapses before he does carry out this duty, the claim for these fees is statute-barred under the Public Authorities Protection Act. With respect to fees under the Notification Act, no such barring occurs, as each certificate is in itself a claim and an account for the fee due, and no further statement is necessary. The local authority has no power to compel medical practitioners to send in an additional account; still less, failing such account being sent in within six months, can they claim that the account is statute-barred. Our correspondent may be referred also to the letter from Dr. Bateman published elsewhere in this issue.

Letters, Notes, and Answers.

☞ *Queries, answers, and communications relating to subjects to which special departments of the* BRITISH MEDICAL JOURNAL *are devoted will be found under their respective headings.*

QUERIES.

E. D. S. desires to hear of any charitable institution which would take in two boys, aged 10 and 13, suffering from Friedreich's ataxy. Their father earns 12s. weekly; he has seven young children.

B. desires to hear of an institution for a lad aged 11 suffering from phthisis and a complaint of the spine; the boy is able to walk about a little. The father is a farm labourer, and unable to contribute towards his maintenance.

*** The case appears to be one which should be dealt with by the county council under the tuberculosis scheme; application might be made to the clerk of the county council.

INCOME TAX.

A. W. is now an outdoor assistant at a salary of £200 a year. Prior to April last he was an indoor assistant at salaries of £132 for one year and £118 for earlier years. The surveyor of taxes wishes to assess him at £200.

*** So long as our correspondent remains an assistant he is entitled to have his income-tax liabilities based on his average earnings for the three years preceding the year of assessment. No tax should therefore be payable for the current year and none for next year, the average income being below £160.

ANSWERS.

SENEX.—Hypodermic tablets are prepared with various bases according to the nature of the active ingredient. As a general rule saccharum lactis is employed; in other cases it may be sodium chloride or sodium sulphate. A minute proportion of antiseptic is sometimes added, but this is hardly necessary. This information is amplified to some extent on page 759 of the *Extra Pharmacopoeia*, fifteenth edition, 1912.

VENETIAN FEVER.

DR. P. DE BLASIO (ss. *Cretic*) writes, with reference to the inquiry by "M.," published on July 6th, that, from his knowledge and from inquiries recently made, he is able to state that there is no special fever peculiar to Venice.

LETTERS, NOTES, ETC.

RHEUMATISM AND PHTHISIS.

AGARICUS quotes in support of the statement that consumption does not affect the rheumatic the following extract from a paper on the morbid diathesis, by Sir Dyce Duckworth, published in the *Practitioner* for January last: "One of the most remarkable features of the arthritic diathesis is its resistance to the inroads of tuberculosis. . . . The bacilli of tubercle will not flourish in a rheumatic soil."

INFREQUENT ACTION OF THE BOWELS.

M. C. W. writes: Dr. Parkinson's letter on constipation (November 2nd, p. 1260) recalls to my mind my great-aunt's "rule of health." My father, also a medical man, often discussed the matter with her. She declared she "never had taken, and never would take, an aperient." She was a spinster in good circumstances, led an active life, and lived very sparingly. She would often go as long as *six weeks* without an evacuation, and *if* at the end of that time she felt uncomfortable, she would put on a leech over the liver, and this always relieved her. Of course all this *ought* to have killed her, but inasmuch as she was 98 when she died, I do not think it did.

A CORRECTION.

IN the JOURNAL of September 28th, p. 794, the letters D.S.O. were printed after the signature of Major F. J. W. Porter, R.A.M.C. It is, of course, unnecessary to say that this was in no way due to Major Porter.

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